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# ALLGEMEINE PSYCHOLOGIE

auf personalistischer Grundlage

von

WILLIAM STERN

Früher Professor und Direktor des Psychologischen Instituts an der  
Universität Hamburg

1935. XX and 832 pp. roy. 8vo. Price 16 guilders (f 2.4.0; \$ 11.—)  
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DR. HOWARD DAVIS SPOERL writes in „**The Personalist**”,  
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logy and the personalistic approach to it, sense, perception, memory, thought and imagination, will and action, including a chapter on character..... and a discussion of Stern's well-known dispositions of feeling. While these contents suggest superficially that the traditional textbook material has once more been set forth, the author indicates by various means that the arrangement is solely for convenience, the treatment always being novel. In the case of memory, for instance, the classical term mneme is utilized in order to discourage undesirable associations with the older psychological view of conscious recollection. Similar devices are employed elsewhere with the same object in view..... A large amount of valuable historical material, much of it drawn from Professor Stern's own remarkable and long connection with organized psychology in Europe, is concisely introduced. There is a classified bibliography of about six hundred titles...."

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*"Rivista di Psicologia"*



MARTINUS NIJHOFF - PUBLISHER - THE HAGUE

### **The Organization of the Organism**

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von

**DR. KURT GOLDSTEIN**

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1934. XII and 356 pp. roy. 8vo. Price 8 guilders (£ 1.2.0; \$ 5.50)  
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The **Journal of Nervous and Mental Disease**, January 1936  
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"The combination of wide clinical and research experience is rather rare at the present day among medical men, but the combination of these two fields of experience with philosophical abilities and outlook is indeed much rarer. Dr. Goldstein presents in this book on the organization of the organism, a conception of the organism as a whole, which is based upon intensive clinical neuropsychiatric experience supplemented by wide laboratory research activities and philosophical consideration. As successor to Edinger as the director of the Neurological Institute in Frankfurt, the author was in a position to pursue extensive investigations on

the physiology, anatomy and pathology of organism of various types. His experience, however, did not end in the laboratory. In fact, this aspect of his experience really led the way for an interpretation of the wider and perhaps more important aspects of his clinical experience. As he himself says, "the drive toward the writing of the book developed from the necessity of treating sick patients".

..... The work should certainly be read by all those interested in the general biological relationships of Neurology and Psychiatry, by those engaged in the practice of special physiological problems, and by those interested in the more philosophical aspects of biological sciences. It presents a wide point of view with an emphasis on observation and experiment, including equally well the fields usually sharply delimited by the neurologist and psychiatrist."

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L'auteur s'efforce de déterminer les lois générales qui régissent l'activité de l'organisme et le comportement de celui-ci..... Il décrit le mode d'édification de notre activité réflexe et la possibilité de prendre la physiologie nerveuse comme base d'une théorie générale de l'organisme vivant; ainsi se trouve soulevé tout de suite le problème des fonctions partielles et de l'unité organique, de la spécificité et localisation, et des antagonismes. La dernière partie aborde le problème psycho-physique: de l'inconscient au conscient, de la norme à la maladie et à l'anomalie, l'auteur montre la hiérarchie des manifestations biologiques, au sommet desquelles se trouvent la pensée et l'action synthétiques. La fin de son livre pourrait être intitulée: à la recherche d'une psychologie du comportement humain. „Journal Belge de Neurologie et de Psychiatrie“.

..... una filosofia biologica che abbandonate le utopie del materialismo monistico, si libra gigantesca sulle ali della ontologia finalistica e creazionistica, della migliore lega. L'opera gigantesca e profonda del Goldstein è stata concepita già da dieci anni; solo ognidì Goldstein, lo ha condotto a termine, creando veramente un sistema di medicina filosofica, fisiologica e naturalistica che può stare a base di una dottrina della vita e della malattia, ognidì più che soddisfacente ed adeguata.

„Archivio Generale di Neurologia Psichiatria e Psicoanalisi“.

181. *Walter Blumenfeld* (University of Lima, Peru) has become well known for his work on the problem of the relationship between optical and haptic space perception. His book "The Relationship between the Optical and Haptic Construction of Space" is a valuable contribution to this field of research. It is a comprehensive study of the problem of the relationship between optical and haptic space perception, and it is a valuable addition to the literature on this subject. The book is well written and clearly presented, and it is a valuable addition to the library of anyone interested in this field of research.

## THE RELATIONSHIP BETWEEN THE OPTICAL AND HAPTIC CONSTRUCTION OF SPACE

by

WALTER BLUMENFELD (University of Lima, Peru)

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## PREFACE

The title of this article might be liable to lead to misunderstanding. When I speak of a „structure” (Strukturierung) or „construction” of optical and haptic space I do not intend to make any statement about a formation or production of „the” space, nor about the way in which space becomes perceptible to us, nor again about the way in which it becomes optically or haptically perceptible. A theory of the origin and production of space „from” something else is in my opinion not a proper subject for scientific investigation and so should not be treated in psychology, however much attention the older philosophy and psychology devoted to it.

My question concerns *firstly* the phenomenal construction of „visual” and „tactual” space. Both structures must be described as accurately as possible with reference to a physical space presumed to be Euclidean. In the *second* place however, the question of the production of these structures arises; that is a question of the physiological processes which correspond to them; in psychological consideration according to the formative forces which proceed from the perceiving subject when he, (as opposed to the objective arrangement of objects in space) produces this very structure in the interests of his orientation. That is the problem of structureformation. Psychology has long been well aware that the organism does not behave passively and receptively during perception. Its activity is however not confined to selection, abstraction, synthesis and accentuation, but must also be regarded to a great extent as giving and altering form. Such performances on the part of the subject also occur in the structure of phenomenal space.

It is now to be assumed that by comparing the structures of optical and haptic space from the standpoint of the laws governing

them, it will be possible to draw some conclusions about the construction. It is possible to make the following preliminary statements.

If the construction depends on only *one* function or group of functions, whether that of vision, touch or any other, even including extra-sensory functions, then the structures must be in perfect agreement one with another on *all* fields of sensation. For if any differences were to occur, they would have to be regarded as particular performances of a space producing type, an assumption which is not in conformity with our hypothesis. On the other hand, the complete agreement of the laws (also from the point of view of their constancy) may be interpreted as meaning that the construction is bound to one single function or group of functions.

If *several* sensory functions contribute independently of each other to the construction, then we ought to expect perceptible differences, since for instance considerable differences exist between visual and tactal sense, according to disposition and accomplishment. If a mutual or unilateral *adjustment* were considered probable, then it would have to be demonstrated by experiments in developmental psychology, and characteristic differences between blind people and those in full possession of the visual sense would be observed.

In any case a clarification of many obscure theoretical questions is to be expected by contrasting and comparing the various spatial structures.

## CHAPTER I

### DETERMINING THE PROBLEM

It is a long-recognised fact that space is perceptually accessible by means of various sense-organs. The part played by the eye has always been considered especially important, whereas that played by touch and more particularly by the other senses is disputed. Occasionally in psychology stress is laid on the unity of the structure and idea of space, even on the „unity of the senses”, just as we get in philosophy the „unity of space”. In general however more attention is paid to the differences, and quite recently the possibility of original tactal experience of

space has been completely denied. In the following remarks we shall be concerned only with optics and haptics and so shall disregard acoustic or other experiences, and not attempt to question their connection with our problem.

Differences in the conditions of perception in both fields are naturally obvious: the eye is affected by electrical waves which occasion experiences of light and colour, whilst the skin is affected by mechanical influences, to which sensations of contact correspond. Although we may assume that the eye is derived genetically from the differentiation of pigment places in the skin enclosing the surface of the body, yet the fully developed organ is different from the skin in nearly every respect. It is an extremely complicated optical double instrument situated at two places in the body, an instrument which may be moved and changed with ease, though not to any considerable extent, and provided with relatively small surfaces which are approximately concave and susceptible to stimulus. On the other hand almost the whole surface of the body, externally of a fairly uniform appearance, although it has many and various bends and turns, is susceptible to tactful sensation. Yet in this function certain clearly defined organs (fingers, hands, tongue) are preferred and these it is true make possible a greater yield, but can only have a moderate speed because of their mass. The co-operation of the two eyes, which develops very early is much closer than that of the tactful instruments which came into being later, although fingers and hands often work together excellently in many ways.

Joint sensations, which admittedly play a very disputed part, may be considered perhaps for touch but not at all as far as the eye is concerned. The retina seems to be considerably more sensitive than the skin to the spatial connections between position shape and size. The optical sense organ yields useful impressions of the surroundings only when we get beyond a certain by no means minute distance from our own bodies, but is effective up to great distances; haptic experiences are possible only in immediate contact with the objects, that is dependent on the length of the arm, in so far as the whole organism does not move. Whereas we can take in a considerable extent of our surroundings simultaneously, without moving our gaze, the tactful sense is limited predominantly to small contact surfaces and a stage by stage making structured the world of objects by means of movements.

Optical space is limited fundamentally by optical qualities-coloured surfaces, tactful space fundamentally *not* by purely tactful qualities, but by the consciousness of the maximum limits of stretch of the organs. The eye conveys to us no experience of the processes taking place in and on it during vision, but only impressions of the appearance of „external” objects; whereas in touch it is possible to relate the impressions either to the touching organ or to the object touched. According to the inner attitude or predisposition we feel either a pressure on the tips of our fingers or the properties of the object touched with the fingers. This twofold attitude even applies when we touch one of our own fingers with another or indeed any skin surface with any organ whatsoever. Every contact place can then occur as „active” or „passive”. We can find in the eye no parallel to this phenomenon<sup>1)</sup>.

It is true that we can see parts of our own body, but that remains a one-sided action and even looking at one's own eyes in a mirror is not to be compared with that phenomenon. We do not see the reflected image where we know or feel our eyes to be.

The objects „present” for both sense perceptions are different: there are things which are visible but untouchable (e.g. reflected images, rainbow, shadows) and invisible things which may be touched (e.g. some currents of air). Finally, we know perspective, two dimensional representations of three dimensional objects in the optical but not in the tactful sphere.

In spite of these differences, there is much to indicate that the connections are by no means slight. The organs of vision and touch both yield impressions of extent, position, direction, shape, rest and movement and both reveal when things are side by side, one above the other or one behind the other. Orientation in space takes place normally without the various sense data contradicting each other and without any gross errors. Even people born blind can learn geometry and talk on equal terms about spatial relationships with those who can see. Thus from the outset the problem of the relations between visual and tactful space must of necessity seem complicated.

<sup>1)</sup> The phenomena reported by H. Werner (Zeitschrift f. Psychologie 114) in which the „observer is himself filled with colour” could occur only in the case of a behaviour „to be termed unnatural and unbiological” (Katz) and are scarcely open to every individual.

The methods adopted formerly in psychology were not favourable to an unbiassed testing of the questions which arise. The tendency to resolve the phenomena into aggregates of associatively related sensations was frequently an obstacle to unbiassed investigation. The orientation around the exact natural sciences led to the mistake of assuming that psychological experiments must be undertaken under conditions which could be termed „well defined” in the sense of *physics*. Therefore on the one hand instead of natural movements the greatest possible degree of fixity in position was prescribed (observation with fixed gaze, touching the skin when the tactual organ was stationary) and on the other hand stimuli were preferred which were in the mathematical sense „simple”, e.g. punctiform. In this way the field which could come under investigation was considerably reduced and in addition the recognition of many fundamental connections was rendered more difficult. Later psychology has recognised that the „pure case” aimed at in experiments is not necessarily identical with one which may be easily seen by means of mathematics or physics. Insistence on a fixed observation and passive reception of optical and tactual stimuli may quite possibly complicate the situation instead of simplifying it. In any case no account has been taken of a fact which may be deduced from the use of speech, namely that *vision and touch are not merely and perhaps not even essentially receptive attitudes of the subject but indeed active (very often actions of daily work)*. From this point of view they even display an unmistakable relationship: we like to describe a gaze which passes over an object as „touching upon it” and we say that someone gives a „piercing” or „caressing” look. We understand *Goethe’s „see with a feeling eye and feel with a seeing hand”* rather as an adequate expression of experience than as poetic license.

In addition there is the following fact: the interest in self observation, in the description of one’s own experience, especially in investigations on tactual sense have lead to the „proprioceptive point of view taking precedence for a long time over the „exteroceptive”. For instance, the „spatial threshold of the skin” was investigated and less attention was paid to the fact that at the same time (even though under restricting conditions) a „minimum tangibile” was determined. Finally, the fact that the question of the individual structure of the haptic arrangement

of objects was less discussed may have been in part due to the prejudice that the „palpable” impressions of the tactful sense were more objectively reliable with regard to the „reality” of the objects than were the visual impressions. This question seemed for a long time to be by no means urgent or difficult, at least in comparison with that of visual space; and when a description was seriously undertaken in conjunction with the problems of the psychology of blindness (Ahlmann, Wittmann), it was not the points of agreement but the differences which made the most powerful impression.

It is a fundamental principle that description has in every science an inalienable right. But the history of the sciences shows us that description gives place to a genetic-conditional method of observation in proportion as ways of arrangement are developed and finally sinks to a preliminary stage. The essential reason for this process, so important for the theory of science, is probably that a phenomenological basis can at the most support a classification or type-formation, which is purely empirical and which always runs the risk of laying down more or less „arbitrary” points of view as bases of arrangement, for the simple reason that no single one can be shown to be objectively preferable. Systems based on pure description possess no absolute *necessity*, whereas those systems built up on conditional-genetic connections have that tendency. For since in that case inevitable conclusions are deduced from the basic assumptions, conclusions which must stand the test of the phenomena, every such conditional consideration tends to become a continuous chain of concrete (objective) links free within itself from all contradiction. And if we go back to the relations between laws, it not only occurs but is indeed frequent that descriptively quite different phenomena reveal themselves as closely related, whilst those which seem very similar turn out to have only a very loose internal connection<sup>1)</sup>.

In spite of the great qualitative differences between haptical and optical spatial phenomena a connection between the *laws* governing them is accordingly conceivable. If it were successfully demonstrated it would be heuristically a useful achievement, for we should thereby obtain a starting point from which we could systematically trace out the corresponding phenomena in

<sup>1)</sup> K. Lewin, Vorsatz, Wille und Bedürfnis, Berlin 1926 p. 19.

both spheres. At the same time it is to be hoped that more tenable hypotheses would be formulated on the considerably wider basis and that the ground would be prepared for a more extensive theory.

As a preparatory I now postulate the following thesis:

*Over and above all phenomenal and descriptive differences, the form of the laws governing optical and haptic spatial perception under materially comparable conditions (up to constants) is identical, so far as our present experience goes.*

In order to explain what is understood here by „comparable conditions” we mention the following facts.

To the whole eye or respectively to the two eyes there corresponds every relatively independently functioning tactal organ of the body, to the retina its affective skin surface, to the fovea its most sensitive place, that is normally the finger tips or the tip of the tongue. To the fixation of the eye there corresponds as state a touching when at rest, or in certain circumstances holding with a motionless tactal instrument as an incident in the process of touching, or grasping with a clasping organ. Related to this in the optical sphere we have accomodation and convergence of both eyes. To successive observation there corresponds a very variable touching and grasping with fingers and hands etc. The „near space” (Naheraum) is of course principally comparable and even in the case of optical perception it will be approximately defined by the reach of the arms.

In accordance with our previous statements we do not intend to discuss analytically the part played by experiences of movement and touch, by joint and muscle sensations etc and therefore the terms „haptic” and „tactual” will not be distinguished here, as is usual, according to the stress laid on processes of movement or of touching. We shall direct our attention to the arrangement, shape, position and size of objects „in space” resulting from optical and haptic activity and to the limiting of the space.

I shall now attempt to demonstrate from a number of fundamental questions that the haptic and optical construction of space is governed by parallel laws.

## CHAPTER II

## THE PARALLEL LAWS

1) It is usually assumed that the single eye, without the use of secondary criteria, perceives only dots, lines and surfaces (not necessarily plane surfaces) and certainly only two dimensions. The third dimension only appears in so far as every object is seen at a distance from the „ego” even though that distance is often indefinite. The parts of our own body are only seen at some distance, which is referred sometimes to the ego and sometimes to the trunk or some other definite part of the body. For tactal perception during gentle touching without movement a corresponding statement is at least partially valid. As long as we exert no force and limit ourselves to touching in repose, we have impressions of points, edges and surfaces, but not of 3 dimensional configurations. The separation from the ego is also as a rule phenomenal when we touch external objects with our hands or feet; even when we touch our own body the ego is usually experienced as being removed into the interior. When „I” press someone to „me”, „I” is not identical with „my I”, the „me” is not the accusative of „my I”. Nevertheless it is correct to say that we think we are touching objects with tactal directness, whereas in the optical sphere a certain distance seems to be insuperable. But we must take into account in this connection that we studiously avoid every direct contact between our eyes and objects. In addition there are a number of phenomena which are probably closely connected with haptic phenomena.

It is reported of people born blind who have undergone a successful operation, that at first the visually perceived images seem so near that they threaten to fall on top of the patients. Cheselden's patient thought that „everything he saw touched his eyes, just as what he felt touched his skin”. Another patient thought that a candle less than 12" from him „was touching his eyes”. Similar statements have been frequently reported<sup>1)</sup>.

Even to the person with normal sight the dark room seems to

<sup>1)</sup> Helmholtz-v. Kries, Physiologische Optik III, p. 182. Hillebrand (Lehre vom Lichtsinn, p. 175 et seq.) attempts to explain these statements, which are unwelcome to him, although they are quite feasible, as „pains due to dazzle” because the objects concerned were (as he says) chiefly „intensively luminous”. But this does not apply throughout nor is such a confusion probable.

extend up to the eyes, whilst its limits tend generally to be 10–40 cm. distant (Katz). More convincing is the fact that the „optical total field”, completely clear of objects appeared to Metzger's subject as „absurdly small and near, like the skin of an egg stretched immediately above the eye” <sup>1)</sup>. The decisive factor is therefore presumably here just as in the case of the dark room and of the people previously blind, first of all the lack of any possibility of a *definite* construction of the field, which places things at a distance from the visual organ only as a secondary and in the case of people born blind in a development which proceeds very rapidly. Jaensch's explanation according to which „in the absence of other localisation motives the visual impressions are localised into the distance of the place of attention” does not explain the impression of touching but at the most that of proximity.

The three dimensionalism of things is immediately obvious binocularly just as in the case of „convergence touching”. From the theoretical point of view, however, further investigation cannot be avoided. For if we accept the doctrine of Hering and Hillebrand on oblique disparation and the stable spatial values of the retina, then it will scarcely be possible to find a parallel law in the haptic sphere or even to consider such a law as probable. I shall return later to this problem.

2) The skin, just like the retina, has besides particularly sensitive places, also those which are difficult to stimulate and those which are almost entirely insensitive. The fovea may be compared to the tip of the finger or of the tongue and the periphery with the surface regions which are only rarely used for touching. There is however no geometrical similarity, nor is there any place corresponding geometrically to the „blind spot”. But functionally connections do exist. The insensitivity of the blind spot does not reveal itself in the normal act of vision even in unocular observation: what is not seen is „supplied”. Correspondingly flat or uniformly bent surfaces and in well known objects even surfaces with complicated bends appear to the hand laid on them as continuous, although inside the surface of the hand and especially between the fingers there exist large „non stimulated” spaces <sup>2)</sup>.

<sup>1)</sup> Metzger, Optische Untersuchungen am Ganzfeld II. Psychologische Forschung 13, p. 10.

<sup>2)</sup> Katz, Der Aufbau der Tastwelt, Leipzig, 1925.

My fountain pen when held in its typical position has tactually absolutely the character of a circular cylindrical object, and my pencil has the character of an angular prism. The surfaces actually touched give at the most but a vague hint of this, as may easily be demonstrated experimentally (e.g. by greasing the touching organs). The fact that in spite of discrete nerve terminations both eye and skin always give us the impression of continuity recedes into the background in face of this less subtle phenomenon but is at least worthy of mention.

3) It is principally the surfaces of bodies which we perceive directly in the optical and haptic near room. In addition there are on both sides phenomena of dots, lines and spaces and under certain conditions, during the real or intended transition into layers of differing depth through material which is either transparent or may be felt through, there are also intermediary stages of various qualities. (Schumann, Jaensch, Katz) Lippert has also proved that empty space can be perceived even by tactual movements.

According to the statements of her subjects it is filled „with something which can be grasped, pressed and touched, with something substantial... From the finger tips of the hand in motion threads flow out... In this self produced inflowing medium the movements are felt” <sup>1)</sup>. It is probable that in this case there exist genetically in both the haptic and optical spheres connections with general laws of contrast <sup>2)</sup>.

4) The much discussed fact of simple vision with two eyes appears to find a parallel in the haptic sphere in the fact that we obtain or maintain the impression of objectivity not only on touching with the palms of both hands, but also with the ten fingers and sometimes with organs relatively even more independent. Here however we must take into account that in simple vision two almost completely coincident retinal images of the same or approximately the same object position are formed, whereas in haptic perception for fundamental reasons identical object positions can only act on various organs in temporal sequence. The individual impressions which are formed simultaneously, analysed out of the totality of their structure, are in general very

<sup>1)</sup> Lippert, Report XII. Congress of the „Deutsche Gesellschaft f. Psychologie”, p. 385.

<sup>2)</sup> Blumenfeld, Urteil und Beurteilung. Leipzig, 1931, p. 127 et seq.

unlike, and in any case differ much do two retinal images which fuse therefore, when referring to simple able to compare those processes of are based on successive acts. Inde worthy of note that several organs uniformly and that on the basis of of perception, in which very diff immediate impression of a unif

5) In the case of abnormal p result phenomena corresponding (Aristotelian illusion) <sup>1)</sup>. Converse feel along the inside of two cords single cord (Ewald), analogous to even get haptic inversion, corresps has shown <sup>2)</sup>. If one runs crossed fi surfaces of a box, the impression is t edges. These parallels too assume They are fundamentally connected cooperation of individual parts of

6) Optically and haptically the thi are distinguished above all others. body is in both cases practically in plane as also basal and shoulder lin and the directions right-left and fr extent. The direction from above that of the weight or respectively v of the field and if displacements of directions generally change unif degree. The trifling deviations w scarcely to be considered decisive that the bending of the head has m cals, whilst the bending of the body verticals, but the sense of the displ only either head or body is displaced simultaneously however, an op

<sup>1)</sup> Cp. also Krass, *Zeitschrift f. Sinnesphysi*

<sup>2)</sup> Drobisch, *Empirische Psychologie*, 2nd ed

<sup>3)</sup> Sommer, *Zeitschrift f. Sinnespsycholog*

<sup>4)</sup> Sachs & Meller, *Zeitschrift f. Psychologie*

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incipitally the surfaces of bodies which we perceive optically and haptic near room. In addition there are phenomena of dots, lines and spaces and under certain conditions the real or intended transition into layers of transparent or translucent material which is either transparent or translucent. There are also intermediary stages of various kinds (Lippert, Jaensch, Katz) Lippert has also proved that space can be perceived even by tactual movements. According to the statements of her subjects it is filled „with something which can be grasped, pressed and touched, with something... From the finger tips of the hand it flows out... In this self produced inflowing medium the spaces are felt”<sup>1</sup>). It is probable that in this case there is both the haptic and optical spheres concurring in general laws of contrast<sup>2</sup>).

discussed fact of simple vision with two eyes has a parallel in the haptic sphere in the fact that we obtain the impression of objectivity not only on the palms of both hands, but also with the ten fingers. In this connection with organs relatively even more independent we must take into account that in simple vision completely coincident retinal images of the same object in the same object position are formed, whereas the organs of perception for fundamental reasons identical object can only act on various organs in temporal sequence. The impressions which are formed simultaneously, in the totality of their structure, are in general very

unlike, and in any case differ much more one from another than do two retinal images which fuse together. Strictly speaking therefore, when referring to simple perception it is only permissible to compare those processes of both fields of sensation which are based on successive acts. Independently of this it is of course worthy of note that several organs in both cases work together uniformly and that on the basis of one single visual or tactal act of perception, in which very different organs participate, the immediate impression of a uniform object can be produced.

5) In the case of abnormal positions of the fingers there result phenomena corresponding to optical „double images” (Aristotelian illusion)<sup>1</sup>). Conversely when two crossed fingers feel along the inside of two cords we get the impression of one single cord (Ewald), analogous to stereoscopic single vision. We even get tactal inversion, corresponding to visual, as Drobisch has shown<sup>2</sup>). If one runs crossed fingers along two adjacent inner surfaces of a box, the impression is that of feeling one of its outside edges. These parallels too assume what has been said under § 4. They are fundamentally connected with the questions of the cooperation of individual parts of organs.

6) Optically and haptically the three main directions and surfaces are distinguished above all others. The symmetrical plane of the body is in both cases practically identical, frontal and pectoral plane as also basal and shoulder line being approximately parallel and the directions right-left and front-back coinciding to a great extent. The direction from above downwards coincides with that of the weight or respectively with that of the resulting force of the field and if displacements occur, then optical and haptical directions generally change uniformly, even if not to the same degree. The trifling deviations which Sommer discovered are scarcely to be considered decisive<sup>3</sup>). Sachs and Meller<sup>4</sup>) proved that the bending of the head has more effect on the optical verticals, whilst the bending of the body has more effect on the tactal verticals, but the sense of the displacement is the same as long as only either head or body is displaced. If head and body are displaced simultaneously however, an opposite displacement of the

<sup>1</sup>) Cp. also Krass, *Zeitschrift f. Sinnesphysiologie* 51.

<sup>2</sup>) Drobisch, *Empirische Psychologie*, 2nd edition, 1898, p. 125.

<sup>3</sup>) Sommer, *Zeitschrift f. Sinnespsychologie*, 61.

<sup>4</sup>) Sachs & Meller, *Zeitschrift f. Psychologie*, 34.

phenomenal verticals may occur, by means of which the optical verticals are determined by the position of the head and the haptical verticals by that of the body. It seems to me that this is obviously because head and body are of differing weight for eyes and touch. Both scholars concluded from this that „visual and tactal space have fundamentally nothing in common”, but taking the „homology” into account I am inclined to take up the opposite standpoint.

7) An object in space has both visually and tactually a direction with reference to the „ego”. Movements have direction „towards me” or „away from me” or „from one object to another”, „from A to B”. But the optical direction from the ego does not coincide with the line of vision of one eye, nor does the haptical direction from the ego coincide with that of the pointing finger or arm. Yet it is reasonable to ascribe a direction even to individual organs. Every single eye has a „straight in front” which is different from the apparent median although phenomenally parallel to it, and the same applies for every other organ. The degree of deviation for such a „straight in front” is probably in general greater than for the phenomenal median, if this is determined by the two symmetrical organs <sup>1)</sup>. Even if an object has considerable extent, yet it is perceived haptically and optically in *one* direction which is determined on the side of the object perhaps by the centre of gravity of its front surface, and on the ego side by a median region or point. In the case of binocular vision, according to widespread opinion it is determined by the Cyclops eye and tactually according to the position of the arms, hands, feet, tongue etc through „points” lying in various directions. In an analytical attitude every point in outer space can be endowed with directions with reference to the ego, for even the parts of one's own body have them in relation to the touching organ and the main axes of the body.

Various objects can lie in „the same” direction relative to individual organs as well as to the ego. If several-especially two symmetrical-organs are working together, then the direction is usually referred to the ego. Hering's „law of identical visual directions” which must be mentioned at this point, is built up on a theoretically interesting experiment and so only governs an

<sup>1)</sup> Fischer & Kornmüller, Zeitschrift f. Sinnesphysiologie 61, p. 96.

artificially produced set of conditions from which the undesired double images have been removed. If we avoid this there can be no grounds for stating that all objects lying for each individual eye on the same line of vision as the binocular fixation point lie phenomenally in the same direction as it.

There is a difference of opinion as to whether it is strictly accurate to assume that one single centre exists for binocular directions of vision. Köllner <sup>1)</sup> by his experiments has made it more probable that variable points on the basal line are to be substituted for it. The situation is similar in the tactal sphere. It seems hardly plausible that a point centre should exist there; and even a linear centre like the basal line in the optical sphere is not probable. The „ego” can scarcely lie within fixed limits, but according to the whole situation in its surroundings it sometimes flows over and beyond the whole body-e.g. when it is directed towards a very large object such as a distant mountain, or even the near by wall of a room- and sometimes it restricts itself to an extremely limited range, which again is very variable. If with my foot I touch a caterpillar crawling along the ground or a wire through which an electric current is passing, then my „ego” is localised in the tips of my toes, and it withdraws to my neck when that part is threatened by a gnat at the seaside. (Compare in this connection the curves of figure 1 page 142 of this article.)

8) When there is intentional movement of the eyes and of the tactal organs <sup>2)</sup> there is a phenomenal repose of the objects observed. Conversely, since Benussi we know also that there are tactal „apparent movements” as well as optical. Neuhaus <sup>3)</sup> compared the two and found complete similarity in 10 out of 13 different points. The following are the deviations:

(a) In visual movements 5 stages can be distinguished, but this does not apply throughout in tactal movements, nor are they clearly connected with the same conditions.

(b) Optimal visual movement appears more readily the smaller the distance from the stimuli; haptically both stimuli tend to merge together if the stimulus is too small.

(c) Visually the distance passed through in optimal movement „always seems greater than it is objectively”, whereas the tactal

<sup>1)</sup> Köllner, Pflüger's Archiv f. die gesamte Physiologie, 197.

<sup>2)</sup> Katz, Der Aufbau der Tastwelt, p. 69.

<sup>3)</sup> Neuhaus, Archiv f. d. gesamte Psychologie, 83.

phenomenal distance seems „as a rule smaller than it is in reality”.

If we neglect the unconsidered formulation of the facts just mentioned, (what is actual size? how can phenomenal and actual size be compared?), there still remains from the last point the discrepancy between the extent of the optically perceived and imagined movement on the one hand, and on the other of the tactually and optically perceived movement. These are metrical questions which must be dealt with at a later stage <sup>1)</sup>. For the rest it is partly a question of phenomenal differences and partly of only gradual deviations which can scarcely make any appreciable difference, especially as other investigators have discovered very exact points of agreement. Scholz <sup>2)</sup> compared in both fields of sensation a distance exposed the whole time with one limited by successive stimuli and discovered that in both fields of sensation the latter always appeared considerably smaller than the continuous extent (distance) whenever those intervals of time occurred in which there was stroboscopic movement. If the distance was greatly reduced then there was both optically and haptically a „superextension”. On the other hand the fact that tactual phenomena of movement apparently cannot be produced with the same regularity as visual ones is in my opinion in need of careful investigation. The comparative rarity with which such movements are perceived in life may play a part in this, but I am not certain if it is the decisive factor.

9) Both in the optical and tactual field there is a form constancy of objects with very varied orientation of the sensory surface quality of the surface (colour – roughness) size and distance. And there can be scarcely any doubt that the general laws of configuration (primate of the whole, signification, transposition etc) of optical objects hold good in the same way for haptics.

10) Great support is lent to this theory by the fact that almost all the so-called „geometrically optical” illusions also appear in the tactual field, as Müller-Lyer and Révész <sup>3)</sup> have shown both for when the organ is at rest and when in motion. Révész has already pointed out that „in the visual and tactual perception of geometrical

<sup>1)</sup> Cp. Hulin, Journal of experimental Psychology 10, 293. In these investigations also the distances of the stimulus points were greatly underrated: the degree of under-rating depended on the time difference. (Criticised in Zeitsch. f. Psychologie 113.)

<sup>2)</sup> Scholz, Psychologische Forschung 5.

<sup>3)</sup> Révész, System der optischen und haptischen Raumtäuschungen, Zeitschrift f. Psychologie, 131.

relations essentially the same processes take place" and that they are to be regarded as „the natural and necessary consequences of our space perceiving function in general". Thereafter he takes up a different standpoint from the one we are expounding here, for he stresses „very considerable differences with reference to the perception contents and the process of comprehension" as well as with reference to the part „which perception can claim to play in the formation of the impression" in both fields.

11) In objective space straight lines running phenomenally parallel to the median plane are lines of concave bending towards the median, which according to the view over the spatial depth run more or less exactly towards the optical central point <sup>1)</sup>.

Analogous tactal experiments can be undertaken by fixing two needles on a board symmetrically to the median plane and attaching to each a short thread 20–25 cm long. The experiment then is to grasp each thread with one hand and to straighten out both phenomenally parallel to each other and to the median. This can be done either by grasping the threads close to the needles and proceeding along them in a proximal direction, when they follow this movement, or by grasping them by the proximal ends and turning them round the needles until the pull of both hands seems to be going in a parallel direction. If this is repeated several times and the averages of the final positions obtained, then the needles can be stuck in again at the points thus determined and continuing pectorally (of course it is also possible in the opposite direction) we obtain two lines which are composed of straight sections and correspond to the well known „Allee curves". It is an advantage to use as the support a large drawing board covered with a sheet of paper and the edge of the board is to be pressed continuously firmly against the subject's breast.

Experiments of this kind show that the course of the resulting lines is not generally straight, but that they diverge towards the subject as long as the fixed distance from the needles is less than the distance from the shoulder joints, but that above this they approach the objective parallels and only gradually begin to converge.

In the following table I we reproduce the allees of 5 subjects. The intermediate values, which do not affect the result, have been

<sup>1)</sup> Blumenfeld, Zeitschrift f. Psychologie, 65, p. 327.

omitted and moreover only the sums of the left and right distances apart from the medians have been shown. In reality the courses of the lines are rarely symmetrical. In figure 1 the curves of subject Te are shown, in this case with the values which were actually obtained for both hands. In all the experiments the plane of the thread lay about 20–25 cm below the shoulder joint. The starting distance from the breast was in every case about 620 cm. All the values are averages from 10 attempts. D denotes the distance from the pectoral plane.

TABLE I

D	Subject Bi					Subject Ja					Subject Kü				
620	100	300	500	700	800	100	300	500	700		100	400	700	800	
420	154	295	489	682	790	123	309	547	685		138	458	807	868	
220	189	321	502	672	775	160	361	567	701		194	504	857	918	
20	217	341	526	683	745	206	372	580	688		246	552	900	949	
Shoulder breadth:	370					390					420				
Upper arm length:	320					310					300				

D	Subject Ro					Subject Te				
620	100	300	500	700		100	300	500	700	800
420	109	283	499	682		189	324	507	682	825
220	134	286	493	672		251	375	540	715	824
20	176	279	463	622		273	422	576	727	790
Shoulder breadth:	330					370				
Upper arm length:	320					300				

The analogy with the optical allees seems at first sight to break down because we frequently get the courses of the lines diverging towards the subject, whereas both my own and Hillebrand's allees are clearly convergent. But if it is correct that in these the direction towards the visual meeting place (Knotenpunkt) is the decisive moment, then we must expect divergences to appear also in the optical field when the starting distance reaches or falls below the pupillary distance. As a matter of fact

Köllner has found such curves<sup>1)</sup>. It is right and proper that the visual distances should not set the standard in tactal space. According to our results it may be assumed that the shoulder and elbow joints and perhaps also the wrists replace them.

In order to test this hypothesis I have had some phenomenal tactal parallels of this kind set up at various heights relative to

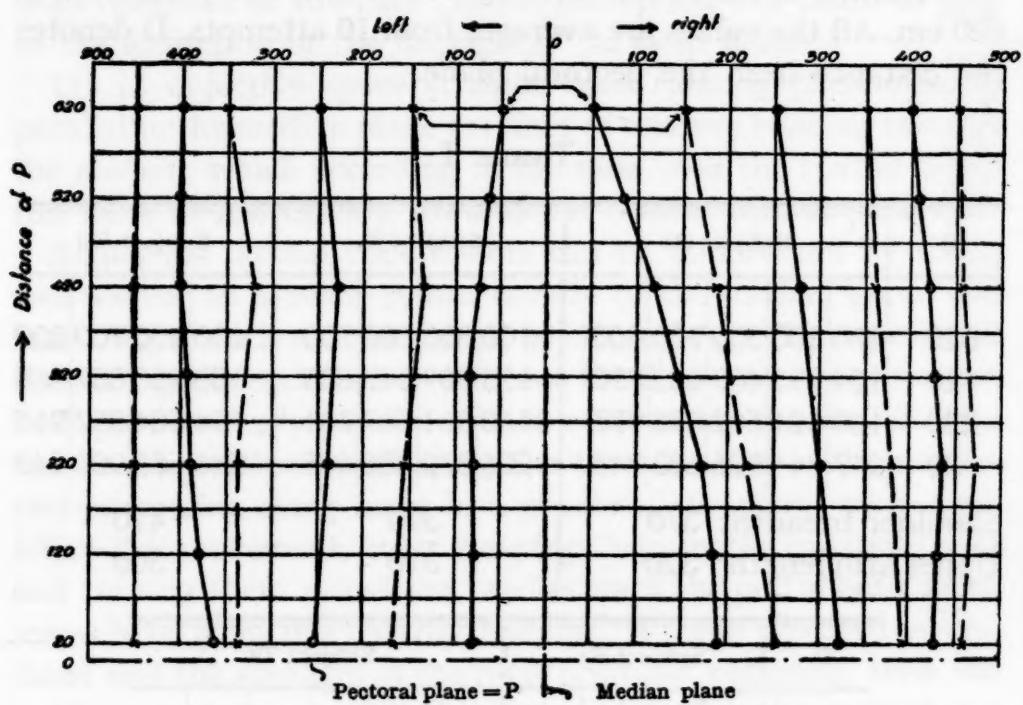


FIG. 1.

the shoulder joint. If the breadth of shoulder is the sole decisive factor then we ought to expect the course of the lines not to be influenced by the conditions of the experiments. But if the elbow joint or wrist plays a part, then when there is a change in the position of the lower arm we ought to get a change in the distance towards the subject.

In table 2, which corresponds to the previous one in all other respects, G represents the height in mm. of the shoulder joint above the plane of the threads. Here too the values are the average of 10 experiments.

<sup>1)</sup> Köllner, loc. cit. p. 532.

TABLE 2

D	Subject Va						Subject Sche					
	G = 600	220	90	600	220	90	630	220	50	630	220	50
620	100	100	100	800	800	800	100	100	100	800	800	800
520	109	124	136	798	812	822	118	120	131	785	796	790
420	132	156	163	793	810	831	137	149	170	762	790	786
320	151	186	191	784	814	838	156	179	223	739	792	787
220	179	218	199	773	819	841	177	223	273	719	788	802
120	196	237	213	759	820	855	198	233	308	682	781	806
20	211	244	228	731	808	834	208	295	361	644	758	799
Shoulder breath:	380									390		
Upper arm length:	300									340		

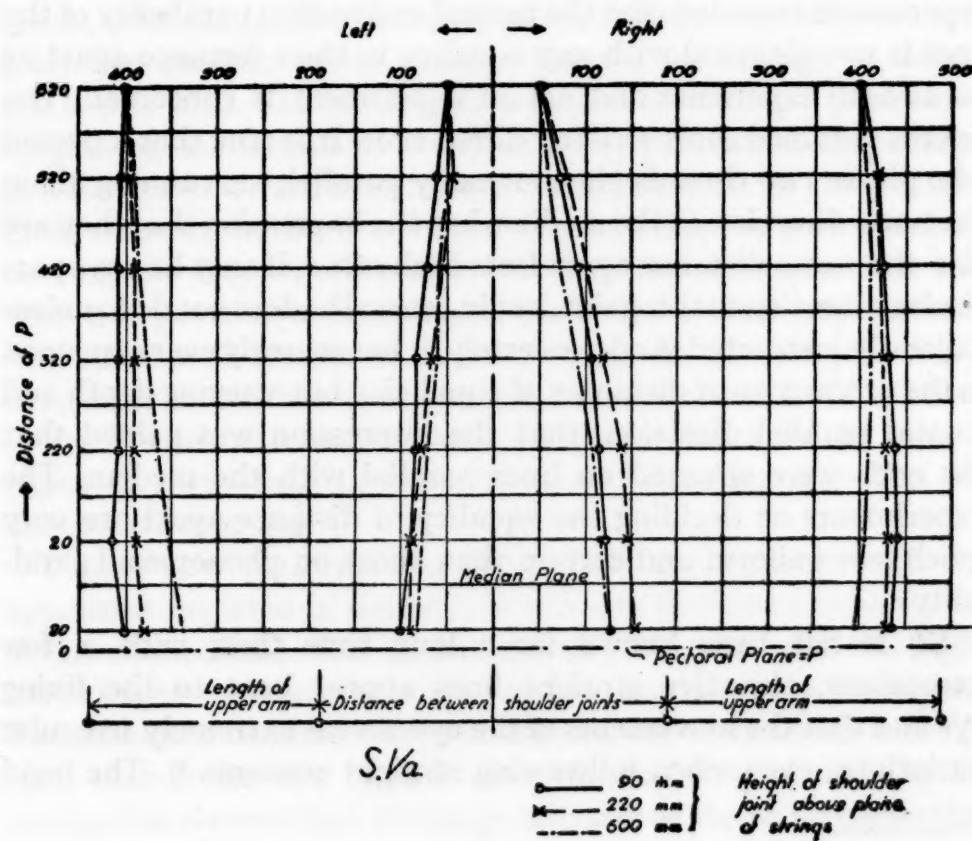


FIG. 2.

From the table and the figure 2 it may be clearly seen that the allee produced standing ( $G = 600$  or  $630$ ) turns out to be the narrowest. In other respects we have once more the typical curves. Some of the striking bends are probably to be explained by the change in the dominance of the various joints.

If we were to make the comparison with the optical experiments from a purely external standpoint we might easily be led to propound diametrically opposite laws. For whereas visual allees become increasingly wider towards the subject, the higher the eyes lie above the plane of the points of light<sup>1)</sup>, we have the exact opposite in the case of tactal experiments. But the points of agreement become clear if the joints are counted as „homologous” with the visual centres of rotation and if we consider that besides the height there is probably not only a change in the influence of shoulder joints, elbows and wrists, but also that in each case the distance of the elbows is decreased with the height of the shoulders above the plane of the thread. Furthermore these experiments revealed that the tactal and optical parallelity of the lines is not identical with any equality in their distance apart as far as both experience and actual experiment is concerned. The figures obtained show striking differences. It is true that a person who places two threads phenomenally parallel, i.e. running along the same direction as the median, has the impression that they are also the same distance apart from each other, if only he pays particular attention to this point, but in general he does not do so unless expressly instructed. And conversely, it has scarcely ever happened in the production of distances of equal size but varying depth and frontal parallel direction, that the impression was gained that the ends were situated on lines parallel with the median. The experiments on deciding the equality of distance apart are very much less uniform and certain than those on phenomenal parallelity.

12) It has been known for a long time that, with a few exceptions, objective straight lines appear bent to the fixing eye and that the movements of the eye reveal extremely irregular variations, even when following straight contours<sup>2)</sup>. The bend

<sup>1)</sup> Blumenfeld, loc. cit. p. 324.

<sup>2)</sup> Hofmann, Die Lehre vom Raumsinn des Auges. Berlin 1920/25, p. 161, 169 & 279.

of the phenomenal parallels, concave towards the median, and the convexes of the equidistants — which the Greeks were probably trying to allow for in the frequently noticed deviation from the straight in the lay-out of their temples — prove moreover that in the case of a moving gaze the same applies as for an eye at rest. Loeb has demonstrated by many examples that this general law also holds good in the tactful field <sup>1)</sup>.

„The straight lines of feeling“ he says „are in general geometrically doubly bent“.

Our tactful allees are also bent and as a matter of fact are in agreement with those of the optical experiments, to the extent that all the courses of lines converging on the subject run concave towards the median: in the case of the diverging lines we do not find this in every case, but it certainly predominates.

13) The „horopter problem“ merits special attention from our point of view. The frontal parallel plane is also bent up objectively concave towards the observer in the optical near space, whereas in the distant space it is bent convexly towards him. The question arises whether there is an analogy to this in the haptic field. The investigation can be most simply performed by placing the middle of 3 rods into the phenomenally pectoral parallel plane determined by the two outer ones. If there is any great distance between the rods this can only be performed by moving the fingers, arms or some other tactful organ. But this does not prevent a comparison with the optical experiments, since it is well known that the bending of the horopter when the gaze is moved has the same direction as in the case of fixation, even though the extent is less <sup>2)</sup>. Experiments with the tactful organ at rest would however be open to serious objection for anatomical reasons.

I have now performed a great many experiments with the apparatus depicted in figure 3, in order to find some explanation of the connections. The touching was performed with the right index finger and the constancy process was the method adopted. The standard distance between the centres of the rods was 210 mm, that is the two outside ones were 420 mm apart. The investigation showed that the shape and size of the rod cross section

<sup>1)</sup> Loeb, Pflügers Archiv 46, p. 32 et seq.

<sup>2)</sup> Tschermak, Pflügers Archiv. 204, p. 220.

exert an influence on the placing. If the rods stand vertically in a frontal parallel arrangement one beside the other, then *in every case* for every subject the centre rod had to be pulled in a distal direction from the objective plane in order to lie phe-

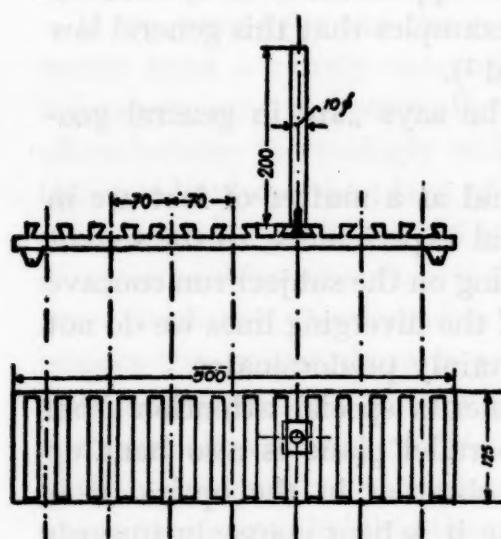


FIG. 3.

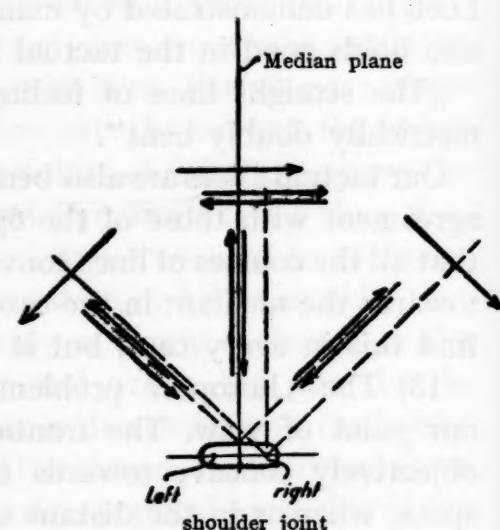


FIG. 4

nomenally in it. But the amount of the displacement varied. On the average of 4 subjects using a square rod cross section of  $15 \times 15$  mm it amounted to 5.4 mm; using one of  $10 \times 10$  mm it was 7.5 mm. With a flat knife shaped steel rod ( $2 \times 10$  mm) which had been slightly rounded at the cutting edge 6.1 mm was recorded and with round rods of 10 mm diameter 12.2 mm. The further experiments were undertaken partly with square rods of  $10 \times 10$  mm and partly with the round rods. The latter are better adapted to the purpose because the striking of the tactful organ on the rounded surface gives fewer criteria for objective movement.

It was revealed that the *direction* of displacement of the centre rod was independent both of the distance between the pectoral and rod planes and of the direction of movement of the index finger. To the phenomenal plane there corresponded accordingly objectively regularly a surface concave towards the wrist of the subject (direction of bend designated in the table as negative). The size of the bend decreased on the average- but not in all subjects — as the stretch of the arm increased. Table 3 gives the data obtained.

TABLE 3

Position of arm	Direction of movement	No. of subjects	No of experiments per subject	Mean displacement	Mean threshold value	Rod.
Stretched	from left	6	7	— 11.1	6.1	square 10×10
Bent	to	6	7	— 11.6	3.6	
Drawn in	right	6	6	— 19.1	4.3	
Stretched	from right	4	5	— 6.4	2.8	
Bent	to	6	6.5	— 8.1	3.5	
Drawn in	left	4	6	— 9.9	3.8	

The same concave bend, in which therefore the surface is pushed out in the direction of the tactful organ, is to be found also in most other orientations; e.g. if the rods represent a frontal parallel phenomenal vertical plane and lie horizontally one above the other, or if they stand vertically and form another of the arrangement represented in figure 4.

Even if the horizontal rods, arranged in a plane parallel to the floor, are touched with the bent or stretched arm, the centre one must almost always be pushed in the same direction. Only in the case of vertical rods standing in the median plane do we get varying values from different subjects, amongst which a very slight convex bend towards the touching fingers predominates.

A comparison of the performances of blind adults and those who can see is interesting. Four blind people of an average age of 24 years placed themselves at our disposal. They had become completely blind when 5-12 years old. The experiments were performed in exactly the same way on them as on 4 non-blind persons. For each type of experiment 10 series of experiments were performed according to the constancy method. The placing of 3 round rods in 4 different planes resulted in the same direction, namely concave in all 8 subjects in 30 cases; only 2 surfaces (in both cases blind persons) showed a slight bend in the opposite direction (height = 1.0 & 0.8 mm respectively). Moreover the concave bend towards the wrist was somewhat greater in the case of the blind people (average 3.5 as against 2.8 mm for those who could see) whereas the threshold values were lower (average 1.08 against 1.55 mm). The blind people tested had therefore

a greater certainty, but their doubtless greater „practice” in touching objectively plane surfaces (walls, doors, boxes, etc.) had a correcting influence on the direction and size of the „ob-

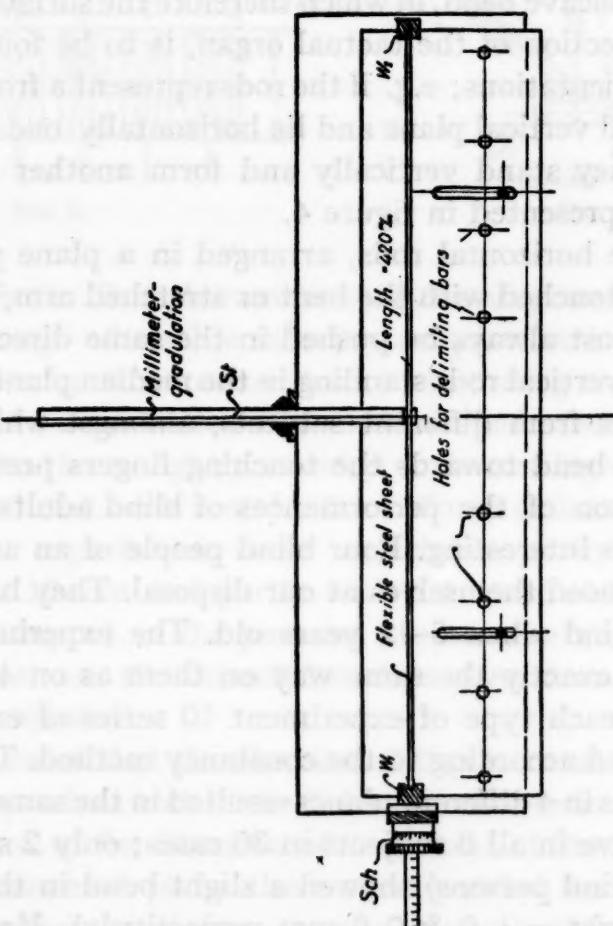
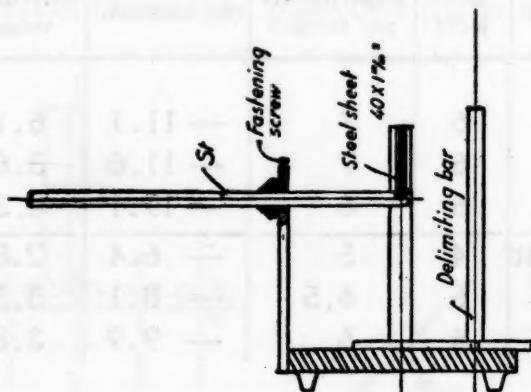


FIG. 5.

jectively false” positioning. Whether this result applies generally would have to be demonstrated by experiments on a very much larger number of subjects.

The occurrence of a concave bend towards the wrist is of course

easily to be deduced qualitatively from the anatomically conditioned circular movement<sup>1)</sup>). Quantitatively we discover admittedly that in the results obtained in our experiments the bend is much smaller than was to be expected in view of the length of the organs engaged. To the (Pfeilhöhe) of 3 mm of an arc whose cord length is 420 mm there corresponds a circle radius of about 7.3 metres, to a (Pfeilhöhe) of 3.6 mm a radius of 3.7 metres and at 20 mm the radius would still have to be 1.1 metres long. The real arc is therefore, as so often in psychology, a „compromise” between the „ideal”, the Euclidean straight line and the result to be expected in accordance with the organic conditions. This intermediate position will be explained in part by a more or less conscious regulation. Self observation in those few cases in which we noticed convex bending actually shows that the subjects attempted to correct the bend which naturally occurs in any completely free and unbiassed action. This probably leads to an overcompensation in the final result.

To what a degree it is possible to make the individual tactal organ independent of the behaviour of the trunk is shown by an experiment in which my subjects were asked to bend the upper part of the body unrhythmically backwards and forwards while they were placing the 3 vertical rods into a pectorally parallel plane 500–600 mm distant from the shoulder joint. Experiments with and without trunk movements were made alternately. On each occasion a concave bend towards the subject resulted. The following table gives the mean values. E = the equivalence value; T = the thresholds and n = the number of the series of experiments (constancy method).

At first sight it is surprising that the influence of the movement of the trunk on the threshold values was not greater. But we may point out that many musicians (e.g. pianists, cellists) when playing are accustomed to make considerable trunk movements towards their instruments. These movements are irregular even though regulated and they vary greatly in intensity, without

<sup>1)</sup> It is difficult to understand how Wundt arrived at his statement: „Through the laws of movement governing the limbs those changes of position are preferred in which the tactal organ moves *in a straight line* towards or on the objects. Since the straight line has thus become the decisive element in the tactal field, this field obtains the form of a plane” (Grundzüge der Physiol. Psychologie II, p. 523). The tactal movements are rather turns (rotations) or derived from them; in this respect too there exists a close connection with eye movements. Jaensch has already noted the bending of the tactal straight lines. (Zeitschrift für Psychologie 41, p. 387 et seq.)

TABLE 4

Subject	Trunk at rest			Trunk in motion		
	E	T	n	E	T	n
Te	— 3.0	3.7	10	—14.5	2.0	10
Ab	—11.2	1.2	10	—18.5	1.8	10
Va	— 9.5	2.1	10	—17.8	5.4	10
Sch	— 5.0	2.3	14	— 5.9	4.1	14

however the dynamic power and precision of the playing being adversely affected. Manual workers too „allow” so to speak for fairly considerable body movement in the resulting course of the movement of their tools.

For the rest, the bending to and fro of the trunk in our experiment produces an increase in the typical bend which may perhaps be ascribed to a repression of the above mentioned compensatory tendency.

It might be objected against experiments with our apparatus that touching in this way without the aid of the eyes creates very obscure conditions. It might be said that it is astonishing that any impression of a plane should arise at all; that the velocity and manner of the contact of the fingers on the middle and last rod depend to a great extent on fortuitous circumstances and may influence the judgement in an incalculable way. But the low threshold values we obtained speak against such arguments & it is otherwise difficult to see why the same direction of bend should happen with such persistence. Admittedly it is quite often necessary to move 3 or 4 times before the subject can make up his mind about the position of the middle rod. For this reason I have undertaken control experiments with an elastic steel band, which can be bent and measured by means of the apparatus represented in figure 5.

In the sketch the steel band is extended moveably between the two counter bearings  $W_1$  &  $W_2$ . The screw Sch makes it possible to bend it outwards, in which case, because of insufficient rigidity, it is necessary to support and fix the band by means of the rod St, which can be fixed in any position by a screw. As this rod only grips the steel band a little from below, it does not prevent tactful movement along the band. Limiting rods can be placed at various

distances and the touching finger strikes against them as it moves.

Ten subjects had to produce the phenomenal pectoral parallel surface by moving along the band (according to the limit method). The length of the tactful path varied. The distance of the band from the shoulder joint amounted to about 500 mm. The result of the experiments was that in this case also a concave bend towards the subject produced the impression of the level plane, although the bend was less than the (Pfeilhöhe) in the rod experiments. Table 5 shows the connection of (Pfeilhöhe) and length of tactful path. The numbers point to an approximately linear connection.

TABLE 5

Tactual path	Pfeilhöhe-
300 mm	— 2.9 mm
200 mm	— 1.6 mm
100 mm	— 0.45 mm

In these positionings also the radius of the circle to which the bend corresponds most approximately (it is more accurately a sinus curve) is 3–4 metres, that is much greater than the length of the arm.

A parallel optical experiment could easily be carried out by fixing three vertical thin white threads to the dark blue steel band at distances of 150 mm each. The surroundings were shut off by a diaphragm and the positioning took place with free vision and at the same distance as the tactful experiments. Out of 5 subjects 4 produced a concave bend towards the eyes, the height of which averaged 3.4 mm. One subject differed in the opposite direction, but unfortunately I was unable to make further investigation into the case<sup>1)</sup>.

The following easy experiment reveals still more simply the tendency so clearly expressed in the tactful experiments. If the outstretched arm is moved to and fro, through not too great an angle, while the trunk is held still and the eyes are closed, then there is a definite impression that the tip of the index finger is moving along a straight line, although it is known that it is describing a circular arc. It is only when the angle attains a very considerable

<sup>1)</sup> Jaensch and Reich have on occasion likewise observed varying horopter positions.

size that the consciousness of a bend arises. An obvious parallel is the vaultlike structure of the sky, which also only becomes noticeable when the head is moved or when there are very extensive turnings of the eye (Filehne)<sup>1)</sup>. Just in the same way, at least as far as I myself am concerned, the intrinsic light and the cavity of the mouth only get their character as hollows when there is movement of the gaze or respectively of the tongue.

Therefore, whereas the parallel between haptical and optical phenomena with reference to the near space can be well supported, there is apparently a clear difference as far as the optical distance horopter is concerned. This, as is well known, is bent in the opposite direction from the near horopter, namely convex towards the subject. In tactal experiments we always discover a concave bend, so far as the direction of the bend might not be changed by the use of very long sounding (feeling) instruments and it is difficult to investigate this question. Nevertheless my experiments permit us to draw parallels in this respect at least, to the extent that the radius of the bend *increases* with increasing distance from the subject. (compare table 3, p. 147).

14) The law of the constancy of phenomenal magnitude applies admittedly in the optical near space to a great extent, if the view of the spatial situation is sufficient, but it does not apply rigidly in every case. The course is, as I have previously demonstrated<sup>2)</sup>, rather complicated when the gaze moves. Maier-Hillebrand<sup>3)</sup> could demonstrate that the obedience to laws presumed by me did in fact apply most exactly to their experiments carried out with fixed gaze: frontal parallel extents of different depth appear phenomenally of equal size if they constitute an equal fraction of the maximal expansion in the surface of vision concerned. Thus it was shown (just as in some of my experiments) that in the immediate vicinity of the body — in contrast to previous assumptions — the more distant stretch must be objectively *smaller* than the proximal, if phenomenal uniformity is to exist. According

<sup>1)</sup> It is interesting that this hollowing of distant plane surfaces is not bound to the direction of the gaze upwards or in the horizontal. According to an essay of Schubert (Pflügers Archiv 222 p. 460 et seq.), when flying straight at heights of 1000–9000 metres the impression is gained that the ground which can be seen is hollowed out and it seems as if the fliers are in the middle of a huge saucer, whose „radius in the horizontal direction is noticeably larger” than in the vertical.

<sup>2)</sup> Blumenfeld, Zeitschrift f. Psychologie 65.

<sup>3)</sup> Maier-Hillebrand, Zeitschrift f. Sinnesphysiologie 61.

Blumenfeld, Zeitschrift f. Sinnesphysiologie 62. Remarks on the above work.

to Maier-Hillebrand this agrees well with many other recorded experiences e.g. with micropsism in Rollett plates, the Aubert-Förster phenomenon, the moon illusion and an observation of Marzinsky.

Yet besides the phenomena just mentioned there are others which can be connected with this theory at least qualitatively. The result of Kundt's experiment in division may be due to the lack of uniformity between the nasal and temporal expansion of the field of vision; it corresponds at least in sense and this idea is supported by certain pathological phenomena.

Axenfeld has pointed out that Kundt's division experiment deviates from the normal in the case of hemianopics, who make „the part lying towards the side of the defect too small”<sup>1)</sup>. In bitemporal semi-blindness the left eye makes the left half too small and the right eye the right half. Jaensch explains this by the underestimation of magnitude viewed simultaneously as compared to those over which the gaze must pass. It therefore seems to me equally possible that for the impression of the magnitude is primarily responsible the total expanse of the field of vision in the direction concerned, reckoning from the fixation point. If a part of the field of vision becomes psychically inactive, that implies a phenomenal enlargement for every distance lying within it.

Some experiments of Loeb give the counterpart in the haptic sphere. According to them in order to be phenomenally of equal size pectorally parallel tactal distances must have different extents according to their orientation to the median plane. This can be connected with the fact that the reach of the arm (the tactal field) in the „nasal” direction, that is crossing the median, is considerably smaller than in the „temporal”, where it can be extended to the full stretch. This also applies if we choose the plane of the shoulder joint as the axis of reference, instead of the median. From the structure and expanse of the field of vision we have a probable explanation of the typical overestimation of the optical verticals as compared to the horizontals.

We may here state our working hypothesis, that *in every stationary condition the expanse of the total field in any definite direction is the standard for the extents of similar direction contained in it*. On going over to another distance the total field usually

<sup>1)</sup> Jaensch, Über die Wahrnehmung des Raumes p. 437.

changes its phenomenal magnitude. From this follow changes in magnitude of all the extents contained in it, unless they are compensated for by an alteration in the magnitude of the retinal images. This compensation seems however to be connected with convergence impulses. If for some reason near vision is rendered difficult, so that increased innervations are required, then micropsy sets in, or if there is difficulty with far vision, macropsy. The connection of both with accommodation on the one hand and convergence on the other is still disputed<sup>1)</sup>; I incline to ascribe the decisive function to the convergence impulses. With greater convergence in the normal act of vision, a decrease of the whole field of vision is naturally connected and an expansion with a small convergence. It is therefore comprehensible that the extents (Metrik) of the visual space are dependent on convergence or on the impulses of convergence, whereas the connection with accommodation alone is scarcely to be understood, since no alteration in the limits of the field of vision corresponds to it.

At the most insignificant indications of tactal phenomena of the same kind are to be expected because the „physical scheme” is very familiar to the human being and the survey of the spatial position of the limbs with reference to distance in general is very clearly present, unless pathological conditions exist. (It would be interesting to investigate the results to be obtained by applying a local anaesthetic to the shoulder and elbow joints.) Nevertheless it was revealed in experiments on adults under normal conditions, carried out by Hilbert at my instigation, that the parallel is fundamentally right. If wooden rods arranged pectorally parallel are compared firstly with arms at breast height pressed tightly against the body and secondly with arms stretched out in the horizontal plane, (naturally without using vision) then the proximal extent must in general be somewhat *greater* than the distal, if it is to appear of equal size. This also applies both if the ends of the rod are held between the thumb and index finger of *one* hand and also if they are held between the index fingers of both hands, although it is more distinct if the distal extent stands temporally in the first place. I communicate here the mean values of 6 subjects, who performed 5 series in both constellations by the constancy process. According to instructions they closed their fingers and

<sup>1)</sup> Cp. Hofmann, *Die Lehre von dem Raumsinn des Auges*, p. 502 et seq.

hands between the principal stimulus and the stimulus of comparison. In the following table 6 the words „distal” and „proximal” mean that in the series of experiments concerned the principal stimulus was first given distally or respectively proximally. The values designate the percentage deviation of the *proximal* extent.

TABLE 6

Length of rod in mm	25	50	100	150	200	250	300
distal. . . . .	+ 10.2	+ 3.3	+ 2.4	+ 9.2	+ 6.3	+ 4.6	+ 3.1
proximal . . . .	+ 0.8	- 0.7	- 0.3	+ 1.3	+ 0.2	+ 3.3	+ 2.3
one handed				two handed			

From the table we can see that the phenomenon is particularly clear in the *small* extents; one handed at 25 mm, two handed at 150 mm, i.e. with a relatively large convergence of the tactful organs concerned. For small extents (100 mm) I was able to note the same phenomenon if an extent limited by rods was passed through with the index finger of one hand at a different distance from the body and if on this basis a comparison of magnitude was produced.

But in a comparison of magnitude in sagittal direction also there exists a qualitatively exact correspondence. Filehne, Pfeifer and Hofmann<sup>1)</sup> found (in opposition to the theory of oblique disparity) that of median depth extents of phenomenally equal size the more distant one must be objectively smaller. This agrees with investigations of Loeb<sup>2)</sup>, who laid down the general law that „in the will to perform movements of equal size the movement performed is all the smaller, the more the muscles participating were already shortened at the beginning of the movement, and it turns out all the greater the more the muscles were lengthened at the beginning of the movement”. It is noteworthy that it is only a question of the degree of shortening and not of the absolute amount of the muscle tension. The same investigator also suggested the same law for the movements of the gaze as for those of the

<sup>1)</sup> Hofmann, Die Lehre vom Raumsinn d. Auges. p. 483 (Special edition).

<sup>2)</sup> Loeb, Pflügers Archiv 41, 1887 p. 107 et seq.

Loeb, Pflügers Archiv 46, 1890 p. 1 et seq.

hands. „There corresponds to the same way of the visual path an impulse which is all the stronger the more the muscles engaged are shortened”. Admittedly from this statement we cannot deduce that the more distant of two optically equivalent depth extents turns out objectively smaller, but it becomes clear that it is formed with a smaller oblique disparation. The fact of „excess correction” lies in the same direction and is covered by the same law. In view of the large number of simple and ingenious experiments of Loeb it would be both interesting and well worth while to carry out exactly parallel experiments in the optical sphere.

15) The phenomena hitherto discussed refer to the general laws of spatial perception as they were observed in normal people. It would obviously be of considerable importance for the question of the inner connection between optical and haptical spatial construction, if *individual* connections could also be proved. Up to the present this has never happened with normal people, but we have some knowledge of it from accounts of pathological cases. Gelb<sup>1)</sup> reports that in his patient Fr an unconscious anomaly of position (tendency to go left) is connected with spatial parapraxes, which proceed exactly parallel in the visual and tactful sphere. In Goldstein's patient Jac and in two patients of Weizsäcker completely corresponding localisation disturbances are likewise to be found. These are connected by Gelb<sup>2)</sup> with disturbances of the normally existing tonus equilibrium of both halves of the trunk. The much discussed case of Gelb and Goldstein<sup>3)</sup> furnishes, so far as I can see, no counter proof. Of course a more accurate investigation from the specific standpoint of my theory would be required. Doubtless we may hope for further valuable explanations of the relation between optical and haptical spatial construction from an exact analysis of pathological cases.

16) Nothing in the tactful sphere seems to correspond to the fact of optical perspective; in any case it is of no importance for perception. But it is easy to reproduce the connection in one's mind by setting oneself the task of marking the places on a level wire gauze at which a finger or a pointer penetrates it when

<sup>1)</sup> Bericht IX. Kongress d. deutschen Gesellschaft für Psychologie, Sammelreferat p. 32.

<sup>2)</sup> Loc. cit. p. 36 & 45.

<sup>3)</sup> Gelb & Goldstein, Zeitschrift f. Psychologie 83.

pointing outwards at a point within reach from the hypothetically assumed „egocentre” (but cp p. 153). The result would be an „image” admittedly not mathematically sharp but yet corresponding in its essentials with the optical image. The more distant would be reduced and „cut across” by nearer bodies which could not be penetrated by the pointer and to a certain degree another motor centre would take the place of the eye e.g. the shoulder or elbow joint or the wrist, according to the size of the sector, yet without any loss to the character of the central perspective.

### CHAPTER III

#### DIFFERENCES AND AGREEMENTS BETWEEN HAPTICAL AND OPTICAL SPACE PERCEPTION

To the naive view of the world the unity of perceptual space is a fact scarcely ever doubted: all things and processes are accommodated in it. In any case it is imagined that optical data clearly preponderate over haptical.

Considering the great majority of parallel laws the working together of optical and tactial perception, so simple to a person possessing sight, seems almost a matter of course. But it should be observed that although the functions coincide the constants do not. Two tactually parallel lines are generally optically convergent and tactually equal distances are optically generally of unequal size: straight lines of one field of sensation will generally appear bent in the other etc. In these circumstances conflicts or mutual adjustments or subordinations of one type of perception to another can occur. It is obvious from general considerations that conflicts are possible only under exceptional conditions and temporarily. Von Hornbostel <sup>1)</sup> has described conflicts of this type by inverted wire figures, Helmholtz evoked them by placing prisms before the eye, Stratton <sup>2)</sup> by means of complicated systems of lenses. And people born blind who have regained their sight by operation can at first combine the two types of impression only with difficulty <sup>3)</sup>. In pathological cases & especially where there

<sup>1)</sup> v. Hornbostel, Psychologische Forschung 1.

<sup>2)</sup> Ewert has recently confirmed the results of his experiments. (Genetic Psychology Mon. 1930, 7, no. 3/4).

<sup>3)</sup> Cp. Wardrop's patient in Helmholtz, Psychologische Optik III, p. 184.

are abnormal tonus conditions, great discrepancies may become evident<sup>1)</sup>.

Since tactal space placed in relation to visual space appears noticeably smaller, it is to be expected that tactually perceived extents would in general have the character of „smallness”, as soon as they appear in the field of vision for the first time. This coincides with Wundt's statements and we experience the same ourselves when for instance we have a blister on the tongue, unevenness or a hole in the teeth, spots of rough skin, grains of sand in the shoe etc. which we first of all estimate haptically and then see. But it would of course be wrong to set up as standard the simple proportionality of heterogeneous total spaces as the basis of extent, if only because of the constant normal cooperation of both fields of sensation. For the person with sight the eye will usually play the dominant part. There are a great number of facts to prove this statement: in mirror-drawing some of Förster's<sup>2)</sup> subjects found that they had the impression that the reflected pencil and the reflected hand were working and not their own hands: „I can govern them but I have no sensation of movement”. Brücke<sup>4)</sup> reports that if one is cutting into a piece of wood with a knife and simultaneously observing the process in the microscope, it seems as though one is cutting into some soft substance such as cheese. Schilder<sup>5)</sup> by suitable fixation produced a double image of a key held upright by his index finger and after a short time, sometimes even spontaneously, he got the „definite impression that two keys were not merely seen but even touched”. „Two fingers are *experienced as being present*”. This feeling of the existence of living fingers is therefore both plastically and essentially dependent on the optical excitation. On the other hand Stratton's experiments show very clearly that under the influence of the real need for orientation the conflict between optics and haptics disappeared and that in course of time the optical directional values (above and below) were *perceptually* subordinated to the tactal and motor values. It is not surprising that in the case of people with normal vision the haptical spatial perceptions usually recede into the background because the visual sense takes in almost simultane-

<sup>1)</sup> Gelb, Bericht IX. Kongress d. Deutschen Gesellschaft f. Psychologie.

<sup>2)</sup> Wundt, Grundzüge d. physiologischen Psychologie II, p. 481.

<sup>3)</sup> Förster, Psychologische Forschung 13.

<sup>4)</sup> Brücke, Zentralblatt für Physiologie 20.

<sup>5)</sup> Schilder, Zeitschrift f. Sinnesphysiologie 16, p. 284.

ously an infinite variety of objects of very differing depth layer. But the predominance of the eye is, as we have said, not absolute and the extreme standpoint which Wundt takes up can by no means be justified. He says „In all cases the tactal sense of a person who can see remains dependent, and even in those exceptional cases where it is reduced to its own resources, such as feeling in the dark, it calls upon the visual sense to assist it”. First and foremost it is not permissible to suppose that tactal ideas are nothing but visual ideas made up of reproductive elements even in active tactal perception in which we use hands and arms.

## CHAPTER IV

### THEORETICAL CONSIDERATIONS

The number of parallel laws in the optical and haptical spheres is so great and the facts are to a large extent so fundamental that it is difficult to believe in chance. However the agreements are not quantitative and the laws prove identical only if certain analogies are permitted, f.e. if the arm and shoulder joints are regarded as corresponding functionally to the visual centre of rotation.

In any case we must reject both the assumption that there is only one single phenomenal *structure* of space — that of the optical apparatus — from which the tactal is merely derived and also the other assumption that haptical space has nothing to do with visual. Accordingly it becomes necessary to make comprehensible how the homogeneousness of the laws governing the construction can arise if the decisive part is not allotted to chance. In the first place the homogeneousness in structure and function of the sense organs will be observed and secondly that of any central processes and then the peculiarities of *one* definite sensory type will be estimated as probably only slightly relevant and in any case only effective to a secondary degree.

By this we push into the rear every theory of space which derives the building up of phenomenal space from *original* properties of individual sensory points or local indications connected with them, as is still frequently assumed for the visual sense. For whereas for instance according to Donders' law the same orientation of the eyes is always applied to a definite direction

of gaze, there are for the same tactal direction very many different arm, hand and finger positions, so that even a complicated system of *fixed* arrangements of position is here highly improbable. In all respects the building up of tactal space seems to be a much more complicated performance than that of visual space because of the greater number of tactal organs which work together in such diverse ways, the irregularity of the surfaces, the variable distances of the tactal instruments and their large amount of independence of each other. Visual space reveals considerable simplification as compared to this. In spite of this certain simplicity or rather because this simplicity of the visual space too easily induces onesided hypotheses, it is probably methodically more correct to start from tactal space. This has the advantage that the phenomena are coarser and so to speak „more obvious”. Furthermore the visual organ is genetically derived from the skin and the experiences of child psychology likewise speak in favour of a temporal precedence of haptical spatial structure: the child „comprehends” things earlier with his mouth and hands than with his eyes<sup>1)</sup>. Opposed to this however the *practical* precedence of the eye for spatial orientation in the mature adult must not be overlooked.

I will now briefly go into the essential coincident characteristics in the structure and function of the sense organs, with respect to the phenomena.

1) In the eye, as in the skin, we find the surface distribution of discrete nerve ends and in both cases the objective arrangement of the elements stimulated corresponds in general to the phenomenal spatial structure as regards arrangement side by side, distance and the direction in the surface; in both cases the „punctual” stimulation corresponds to a flat surfaced continuity. The clearest possible perception is given haptically and optically by places with the thickest accumulation of sensory cells.

2) The forms of movement both of the eye and of the tactal organs are either turns or composed of them or derived from them. And yet more intricate phenomena occur in touch than in vision,

<sup>1)</sup> Even as early as Aristotle tactal sense is the „basic sense” of which all the others are merely derivatives, the hand being the organum organorum. Thomas Aquinas (De Anima II, 9) points out that man of all living creatures has the most highly developed tactal sense and supposes that the people endowed with a finer tactal sense are at the same time those who are superior to others in intellectual understanding. (Söhngen, Sein und Gegenstand, Münster 1930, I. 131 et seq.)

even if we take into account the combination of eye movements with those of head and trunk. Straight line movements can only be realised by roundabout means. To this we might ascribe at least in part the bend of most phenomenal straight lines (parallels, horopters) and the „orthogonous localisation tendency”. The objective straightness of median stretches corresponds, if using symmetrical organs, to the outline of a parallelogram.

3) Since all (including eye) movements (in the sense of definite *performances* of the whole organism) are set up and governed by muscles, it will be necessary to think of the part possibly played in the construction of space by their innervation, tension, shortening and expansion, above all by their cooperation, but also by their anatomical and physiological properties. We imply of course without them having to be necessarily consciously noticed <sup>1)</sup>, as the former polemic against „innervation sensations” assumed. Alterations in the state of the muscle positions occur even at the outset of a will impulse, that is on preparing for a definite movement <sup>2)</sup>. It is to be assumed that processes also take place in the central organ which are not dissimilar from those produced when movement is actually performed. If this hypothesis holds good we have an explanation of many of the facts which have been taught in psychology about the importance of the „wandering of attention” <sup>3)</sup>. Peripheral optical perception is then not *fundamentally* different from that with fixed gaze, especially as we regularly note corresponding impulses to displace the gaze introspectively when fixing or observing a peripheral object. Thus it becomes comprehensible that the horopter and the optical and tactual spatial illusions, as also the phenomenally parallel straight lines in a stationary position (apart from constants) obey the same law as in the case of visual and tactual movement. It is also worthy of note that a connection of the individual muscle activities with the tonus conditions of the whole organism must

<sup>1)</sup> Katz also claims objective movement as a purely causal factor for the genesis of the impressions of roughness and smoothness. (Der Aufbau der Tastwelt, p. 245.)

<sup>2)</sup> Jacobson, Electrophysiology of mental activities (American Journal of Psychology 44). According to him, if movements are only imagined there are produced in the muscles concerned electric tensions which correspond to those produced during real movements. Cf. further Wundt's theory of the „central components” of the sensations of movement (Physiol. Psych. 5th ed, II p. 38) which „can probably also be termed the reproductive elements of the sensations of movement”.

<sup>3)</sup> v. Monakow, „maintains throughout his works the postulate that the perception of space must stand in a fundamental relationship to the impulses, that is to motor processes”. (Jaensch, Über die Wahrnehmung des Raumes, p. 88).

be assumed, a connection which has its effect on both optical and tactal perception.

Révész in his latest work has considered the part played by „kinaesthetic impressions” in tactal spatial illusion and states that they play „a leading part”. He also considers it possible that they bring about a strengthening of the illusory effects <sup>1)</sup>, although he disputes that impressions of movement and impulses to movement are of „fundamental importance” for their origin. So far as judgement is based on „time differences” it is, he says, a question of „judgement illusions” and not of „perception illusions” Moreover, he states, homogeneous phenomena exist even if all movement is excluded and the tactal sense can not bear the responsibility for the „autochthone illusions” which are bound to the impressions of the sense of movement.

I cannot subscribe to this conception. As far as the idea of the illusion of judgement is concerned I have already set out the differences in my point of view in another place <sup>2)</sup>. The strict division between tactal and haptical impressions, the importance of which for the phenomenal side of the problem I do not overlook (cp p. 136) must in my opinion, in view of the theory of the phenomena, give way in favour of genetic points of view. It is of course not to be overlooked that touching with organs in repose, just as judgement of optical objects with a fixed gaze, are rare & unnatural methods of behaviour, the outcome of which can depend on original experience gained from moving organs. We must also bear in mind that in the original behaviour the movements of the limbs need not necessarily be conscious. For the rest, I consider it not quite free from objection to ascribe a strengthening of illusory effects to kinaesthetic impressions and yet to dispute a fundamental importance in the origin of these effects.

But it could be pointed out that the eye can perceive no forms at all during its movement, but only in the rest pauses between the twitches, as the experiments of Erdmann and Dodge on reading have demonstrated. On the contrary in tactal sense it is essentially movement which releases the „creative power” <sup>3)</sup>. I am however not quite convinced that such a comparison is justified. For the „creative power” is revealed in tactal sense essentially in the

<sup>1)</sup> Loc. cit. p. 365.

<sup>2)</sup> Blumenfeld, Urteil und Beurteilung, p. 134.

<sup>3)</sup> Katz, Tastbuch, p. 62 et seq.

judgement of degrees of roughness and hardness, that is, it is only comparable with the apprehension of surface by the eye. As far as I am aware it has not yet been investigated if the colour and quality of a surface are not recognisable to the moving organ. And it has also not yet been proved whether in the exact analysis of tactal processes the recognition of form is not also bound to the typical to and fro movements or respectively to the temporary cessations in the case of tactal *twitchings*. But even if that should not apply we shall in my opinion have to stress psychologically that in experience the objectively twitching eye movement appears just as continuous as that of the tactal organs and that a uniform course probably corresponds to it centrally as well.

4) The tactal structure of space is of a predominantly successive nature. To that extent it differs from the optical, which is to a high degree complete, even during the momentary act. But the visual idea of space can also be formed successively and the effect seems to be largely independent of the way in which it originates. Thus no exaggerated value is to be attached to this difference between optical and haptical perceptions. Mnemonic traces can replace direct action. As a matter of fact there is often no clearly marked dividing line. In the optical sphere the visual space passes over without phenomenal differences into the „merely” imagined space above our heads and behind our backs. In the tactal field the differentiation of perceptive and imaginative space can either not be performed at all or only arbitrarily and artificially. The structure of the central organs must accordingly be so assumed that in the phenomenal space the relicts can combine with the perceptions to form a unit.

5) However there still remain over and above this other psychological moments which are of importance for the building up of both visual and tactal space. In *localisation* the consciousness (more or less clearly defined) of a „physical ego”, the presence of a „physical scheme” exerts an influence. The subjective „idea” of one’s own body according to position, extent, attitude of the limbs<sup>1)</sup> goes into the perception, an idea which seems to me to be neither exclusively tactal nor exclusively optical. The body is —

<sup>1)</sup> I have placed the word „idea” (Vorstellung) in inverted commas because the very complex phenomenal basis is perhaps to be understood in another sense than as an „idea” and moreover an indefinite one. But it would scarcely be more correct to substitute „knowledge” or „consciousness” for „idea”....

chiefly in an upright position — the starting point of a reference system concerning *directions*. Only in this way can statements about above and below, right and left, front and back, have any meaning. We have already mentioned (cp p. 146) that the starting „point” is not to be thought of as made up of points and also that gross illusions are by no means out of the question in judging the localisation of the whole system of directions. In addition we must also take for granted in general a capacity to *transform* the habitual coordination system in certain limits. Almost every place on our body can become the reference point of all directions and even definite directions of the trunk or of individual organs are distinguished, as G. E. Müller first taught for definite cases, without us knowing anything explicit about it, and the body position is in these cases more or less adequately attended to. Thus the shape constancy of objects seen and felt can be made comprehensible when displaced relative to the eyes or the tactful organs, as can many alterations of direction when the body position changes. At the present time we cannot express much more than vague suppositions about the origin of the „physical scheme”. It is probably developed in early childhood on the basis of haptic experiences and later in close conjunction with optical experiences and probably is subject to constant alteration during growth<sup>1)</sup>.

6) The depth relief of objects in space offers perhaps most difficulty for the theory of the construction of space. Proceeding from the phenomenal tactful field it is usual when directly touching objects for a greater distance from the ego to correspond to a greater stretching of the arms and fingers and for an approach to correspond to increased bending. If we run a finger along any bent surface, keeping the pressure constant, we shall notice the differences in distance by an alteration in the position of the organs, based on the physical scheme and the processes of movement governed according to the constant touching pressure. As far as our impression goes we touch objects directly with the tactful organs. But we also get information about objects through the agency of sounders and in that case we think that we feel the objects themselves, and their surfaces so to speak through the

<sup>1)</sup> Cp. in this connection Katz, *Zur Psychologie des Amputierten und seiner Prothese* Leipzig 1921, p. 39.

sounders. According to the investigations of v. Skramlik<sup>1)</sup> however, if all criteria concerning weight, lenght and probably also the shape of the individual sounder are excluded, *the objectively correct localisation* of the object touched by it is absolutely *impossible* and the subjective impression is totally dependent on the psychical attitude. But further experiments by the same scholar and particularly some by Niehaus<sup>2)</sup> showed that if the qualities of the sounders are known or can be deduced by an auxiliary means, it is possible for both people with sight and those born blind to give extremely good performances with them. Indeed we also feel with our teeth and our nails and animals feel with their fur and bristles, which in themselves are not susceptible to stimuli. It therefore seems worthy of consideration whether *all spatial perception* should not be considered as fundamentally *an operation with sounders*. We can immediately go a step further along these lines if we consider the organs of the body themselves as „sounders“ of accurately known lengths, in some degree projections of the originally active nervous central organs. In conformity with this, people who have had a limb amputated place the experiences of touch for some time after the operation in the extremities which are no longer there and only gradually do they refer them to the stumps. It is therefore to be assumed that the persons must gradually „get accustomed“ to the changed sounding lenght and construct a new physical scheme in which a *shrinking* of the „phantom member“ usually occurs. This is also in agreement with the otherwise incomprehensible fact that in the experiments of Katz the spatial threshold at the stump of the arm is smaller than at the corresponding places of the whole limb. The distance which must separate two points in order to distinguish them amounts on the average to 72% of that for the whole arm<sup>3)</sup>. Katz is of the opinion that peripheral conditions scarcely come into consideration but solely central ones and points to the greater attention typically paid by such persons to the stump, as compared with the uninjured members in view of the fact that we have known for a long time that the spatial threshold is dependent on the attention. But perhaps the greater sensitiveness is rather to be attributed to the fact that in consequence of the

<sup>1)</sup> v. Skramlik, Zeitschrift f. Sinnesphysiologie 60, p. 262.

<sup>2)</sup> Niehaus, Zeitschrift f. Psychologie, 122, p. 75.

<sup>3)</sup> Katz, Zur Psychologie des Amputierten und seiner Prothese, p. 56 et seq.

increasing dexterity with which the stump is used as a sounder, central functions more easily play their part even without an increased duration of attention. If we define as a sounder any practically rigid object serving for touching, at whose distal end the object felt is localised, then tactual spatial perception is nothing but sounding.

Touching with individual sounders of unknown length and shape (not rigidly connected to the organism) permits no localisation as long as it can only be acted on in *one* direction. But if a single sounder, held as firmly as possible, on a cone-shell is turned around its point of support<sup>1)</sup> or if two sounders are used for convergent touching, the localisation is immediately improved. By successive processes of this kind we can work out the form of an object and its spatial relief. Here is not the place to show how the touch of blind persons especially is built up on this foundation. We are only interested in its connections with optical perception.

A direct connection with the corresponding visual process meets at once with difficulties, because nothing corresponds optically to the control of the pressure of touch, unless perhaps the alteration of the accomodation, which is, as is well known, only slightly effective. If it is desired to orientate optical perception around the process of haptic perception it would have to be assumed that the rays of light can act as sounders with which the optical apparatus acts, in the same way as the tactual with solid rods. Before we reject this conception<sup>2)</sup> a limine, admittedly rather strange at first sight, it should be considered that there is nothing unusual in a description of an experience in which we term the searching observation of spatial conditions as „touching upon” (cp p. 133). Especially when it is a question of unknown, small or very distant objects, or those seen in a mist whose shape concerns us, we have as a rule the impression that we are *active* and that we are using our eyes as tools for the purpose of touching. A similar conception held almost complete sway in antiquity and the middle ages. This light sounder remains a strange conception because of the non-material nature of light and the lack of a solid link with the eye.

<sup>1)</sup> v. Skramlik, loc. cit. p. 266.

<sup>2)</sup> „Vision is to be regarded as incomplete touching, projected however into space, whereby the rays of light are used as long feelers”. Schopenhauer. Über den Satz von der vierfachen Wurzel § 21.

The physicist could recall that according to the modern conception the material of tactful sounders too is only considered to be electrical energy. But that will, I fear, by no means satisfy the psychologist. The other argument also will convince few people, that according to the general theory of relativity „the interval between two parts of the same ray of light is nil, so that its beginning and end point may be considered as in contact”<sup>1</sup>). If we could assume this, then the retina would be in immediate contact with the objects emitting and refracting light and we should be touching things directly with it. And it would need only a small step in the same direction for us, in accordance with the foregoing to consider the retinas themselves as sounders of the brain.

If, in spite of such considerations we still feel a violent resistance to such a theory, this may be due amongst other things to the justifiable desire, not satisfied by the theory, to get to know the physical or psychical processes which take place during feeling with sounders. These are admittedly just as obscure in the haptical field as in the visual. This much however seems to emerge from our previous enquiry, that the *convergence mechanism* is of special importance for localisation. It is therefore urgently necessary to decide this question, if parallels are to be drawn. If we bear this in mind there is very little to be urged against the term „light sounder” because of its connection with the real process.

If no criteria are recognisable for the fundamentally varying length of the rays of light proceeding from an object to the ego, then a definite depth localisation is impossible, or respectively dependent on ideas and chance tendencies. Hillebrand's edge experiment (Kantenversuch) reveals this for unocular perception under certain conditions; it also shows that even a definite state of convergence and accomodation of *both* eyes is not sufficient for definite localisation, unless a real light excitation of both retinas is present. If head movements are permitted — which correspond to the turning of a material sounder around its supporting point on the outside of a cone (successive convergence) — then even the unocular apparatus acts fairly accurately. With binocular observation — corresponding to touching with convergent sounders — we have in general directly the definite localisation.

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<sup>1</sup>) Russell, Philosophie der Materie, Leipzig & Berlin, 1929, p. 129.

The phenomena in the optical total field (as in the dark room) and with operated blind people show however that when any clear division of the field, still contained in Hillebrand's experiments, is no longer present *in spite of* the existing convergence and binocular perception, the impression arises that what is seen is touching the eyes. The experience of any „distance” at all is therefore not dependent on the presence of a state of convergence, or at least not on that alone, but presupposes a structured field of vision or the possibility of a structurisation of a field of vision, since only this can assure the appearance of a fixation of *the same* point with both eyes.

According to the theory of Hering and Hillebrand the „apparent place of a visual object relative to all others” is a „definite function of the pair of congruent points or respectively of each individual point of such a pair”, without anything being thereby determined about the localisation of the whole field of vision<sup>1)</sup>. By „place of a visual object” we understand however on the one hand the relation to the „Kernfläche” and on the other hand the direction relative to the main visual direction. Both are included in the theory. „Congruent points” are points „of identical visual direction”, but their totality determines also the system of the points seen simply which lie in the „nucleus surface”. Such corresponding points of the retina according to Hillebrand retain this property independent of the state of convergence of the eyes; that is they have „stable spatial values”.

We shall next deal very briefly with the problem of depth perception, omitting all question of extent (visual size, -depth and distance).

If we start from the usual premise that in the starry sky all objects are seen binocularly simple and in the same direction as unocularly, then they ought to lie on the horopter. In connection with the stability of spatial values it follows however mathematically that the horopters are curves at whose points the lines of vision cross at a constant angle and thus possess an equal degree of convergence (equal parallaxes). These are the Müller horopter circles. But the stars, although seen simply, do not lie in the nucleus plane. According to Hering the „empirical horopter” is found if the results of Kundt's division experiments are taken

<sup>1)</sup> Hillebrand, Zeitschrift für Psychologie, 54.

into account. It has not yet been possible to determine the curves mathematically, but there can be no doubt that *it is equally justified to define them through the convergence of the lines of vision as the congruent points*.

It is however psychologically clearer to refer the depth perception to processes and states of convergence, especially as a whole series of facts is in its favour. The „Kernfläche” impression comes to a number of points if the convergence of the lines of vision corresponding to them obeys a definite law. Deviation from this law (oblique disparation) gives rise to movement impulses if there is a corresponding psychical attitude and these impulses need not necessarily be noted by the consciousness. They act however in the same way as the impression of a difference of depth, or correspond to it phenomenally. As far as the theory of shape is concerned, it is easy to assume that somatic states of equilibrium correspond to the „nucleus plane convergences”. We need not go into the question of whether it is necessary to assume that the depth impression is primarily connected with „wandering attention” (Jaensch) if we regard as probable a functionally close connection between these and the convergence impulses.

If convergence movements and impulses determine the localisation of depth, then the form of the horopters can be understood at least qualitatively, by taking the following hypothesis as a basis:

a) there exists a tendency to maintain the state of convergence present in fixation, a tendency which is revealed in corresponding impulses, in the production of the nucleus plane (circular shape) but also in the sense of „orthogonal localisation”;

b) independent of this there is a general tendency to produce a tonic optimum of the muscle system. Now the rest position of the eye muscles is in any case one of small convergence (in death even divergence). Such a tendency must therefore reveal itself in the direction of a flattening of the Müller horopter circles as is indeed actually observed.

Tactually an exact parallel would be realisable only in experiments in which a phenomenally frontal parallel plane is produced with two sounders of unknown length. Such experiments have not been performed, for they correspond to no natural behaviour. And we must also take into account the complications which are

caused by the various joints and the diffuseness of the idea of a tactful „convergence” thereby produced.

When in our experiments in which only one finger was used the same direction of bend of the phenomenal straight line appears as in the case of the near horopter, then that is connected with the anatomically conditioned turning about the points of the joints, corresponding to the movement of the eyes; the flattening of the curves is probably, either wholly or partially different from that in the visual field, to be explained by conscious correction (cp. p. 162) For the bending of the phenomenal parallels it is probable that both haptically and optically the orthogonal localisation tendency which partakes in the movement as a component is responsible.

Therefore although in these latter cases the process takes a different course in the tactful building up of space from that in the optical, a connection can be realised by introducing the sounder conception. But in this case it must always be remembered that on the optical side there is solely the convergence mechanism as *a real corresponding basis*.

Perhaps it will be thought that the influence of the accommodation is not taken sufficiently into account here. As is well known, it is closely connected with the convergence impulse -at least in the normal act of vision. If it should be necessary to define the part played by it in the special parallel, we could repeat that without a suitable accommodation no sharp image can be produced on the retina. The „light sounders” then lack a clear limitation; they allow the eye so to speak no fixed comprehension and correspondingly they disturb certain localisation.

It seems to me important to point out that in the conception here adopted account is taken of the impression of active touching even in the case of optical perception. But more important still, it also becomes clear that the phenomenal spatial structure results *dynamically* by the activity of organs, but not through the accumulation of a series of points, whether these are in relation either directly or indirectly to the retinal or cutaneous elements.

This conception agrees moreover very well with certain experiences in animal psychology. Von Uexküll<sup>1)</sup> tells of the

<sup>1)</sup> v. Uexküll, *Umwelt und Innenwelt der Tiere*, Berlin 1921, p. 172 et seq.

shafted facet eyes of arthropodes, the structure of which „resembles a movable tactal organ, which can take in many impressions simultaneously and is therefore probably adapted to feeling not merely individual objects but also the space intervals between". In discussing the spatial perception of crabs whose antenna possesses only a single touching bristle, the same author mentions that this movable tactal hair has the same effect as 100 motionless tactal hairs which are attached to the surface of a round shell of about the same surface of action. The same applies for animals which „have only one single retinal element at the tip of a movable eye shaft".

7) The space content (Metrik) of spatial perception demands and deserves detailed discussion in this connection. It must also be brought into harmony with the conception developed. Therefore it is a question of phenomenal magnitude in the nucleus plane and in the depth dimension, in which case also we have especially to take into account the visual distance (absolute localisation).

In the previous remarks (p. 167 and p. 171 et seq.) two principles are mentioned which can serve as guides.

I In every stationary state the expansion of the visual aggregate field in a definite direction is the standard for the extents of like direction contained phenomenally in it.

II There exists a connection between the amount of the movement as compared in size with the corresponding impulses on the one hand and on the other hand with the degree of shortening (not of tension) at the beginning of the experiment of the muscles participating in a movement.

Now the first statement refers originally only to fixed optical gaze. But we have had reason, supported by a series of experiences, to consider observations with fixed and moving gaze as fundamentally equal. If the thesis of the „identity" of haptical & optical construction of space is to apply, then the first statement must apply also for resting and moving tactal perception, and it must be possible to produce a connection between both statements. Then a connection would have to be assumed between the expansion of the perceptual field in all dimensions (optically as well as haptically) and the motor processes and impulses. Jaensch has maintained a fundamental equality of the three optical dimensions and supported his view with cogent reasons. Fischer<sup>1)</sup>

<sup>1)</sup> Fischer, Handbuch der normalen und pathologischen Physiologie XV, 2 p. 1010.

discovered from careful experiments with the haploscope that the apparent magnitude was „a simple linear function of visual distance”, in which case this itself depends on convergence.

Some further experiences supporting our thesis have been reported above (p. 167 et seq.) But in order to prove the correctness of this conception a whole series of more specialised investigations would be needed and these are rendered difficult on the one hand by the strong and little perspicuous mutual influence of optical and haptic processes and on the other hand by the intimate connection between visual magnitude in the nucleus plane and visual distance and visual depth, especially where the visual distance is great.

#### CONCLUSION

According to the teaching of modern physics the shape of objects depends on the spatial extent (Metrik) prevailing in the field concerned. If we apply this to phenomenal shapes, it is obvious that visual and tactal things appear differently from what corresponds to measurements with the usual physical methods. In numerous shape illusions this state of affairs appears clearly, and perhaps most clearly in the phenomenal straight lines as the „simplest” shapes. These are in general bent „objectively” and vice versa; three points of a physical straight line do not usually lie on a phenomenally straight line neither in the visual nor in the tactal field, even if any desired organ movements are permitted. And the amount of bend varies according to the position and distance in the field.

We may be permitted to assume that the form of curves depends on the whole psychophysical field of force, which controls the movement and even the movement impulse. Then this theory corresponds to the one laid down by Heinrich Hertz in his „Principles of Mechanics” as the only physical law of movement. This states that in the absence of compelling forces every body is describing a „geodetic line” in a quite clearly defined direction, i.e. the shortest possible on a plane taking into account the existing field forces. Or in other words: the line passed through at any time is in a correspondingly „bent” space always a straight line.

If the extent of tactal space is not identical with that of

visual space and both differ from that of Euclidean space, no false conclusions must be drawn therefrom concerning „objective” physical space. Visual and tactal space result when there applies in physical space „that extent which we place into it psychologically (and which of course will be different for tactal sensations than for visual sensations)” (Reichenbach). This conception must, it seems, be drawn from the facts. The spatial arrangement of objects can reveal a varied phenomenal shape, according to the kind of standards applied, which are conditioned by the concrete sensory motor processes and organs. Obviously, however, the objectively concrete connections must somehow reveal themselves, even in perception, in such transformations. From experience the various phenomenal spaces-visual, tactal and auditory — have in common *the* moment that they are drawn into connection with human *conduct* and are constructed by means of its activity. The human being is orientated by and acts according to the things and processes in space. But *how* they appear to him, that is not independent on his actions. In this sense we understand to-day the strange sentence of Duns Scotus „Videsne itaque non aliud esse locum nisi actionem intelligentis atque comprehendentis?”

#### RÉSUMÉ

L'auteur compare la structure de l'espace visuel et celle de l'espace tactile non pas au point de vue descriptif, mais d'après les lois aux-quelles la perception des phénomènes de l'espace est soumise. Il fait observer que, par une considération conditionnellement génétique, on voit qu'à chaque loi du domaine de l'un de ces sens, correspond une loi semblable du domaine de l'autre, et il appuie cette assertion non seulement sur le grand nombre de phénomènes normaux et pathologiques connus, mais aussi sur de nouvelles expériences faites par lui. De cette comparaison naît la conviction que, tant dans le domaine visuel que dans le tactile, les facteurs moteurs et dynamiques participent d'une manière décisive à la structure de l'espace, et que, en conséquence, la perception visuelle de l'espace doit aussi être conçue comme si on pouvait le toucher à la sonde. Il se présente également quelques données concernant la mesure de l'espace phénoménal.

#### ZUSAMMENFASSUNG

Der Verfasser vergleicht die Struktur des optischen und die des taktilen Raumes nicht deskriptiv, sondern im Hinblick auf die Gesetze, denen die Wahrnehmung der räumlichen Erscheinungen unter-

liegt. Er zeigt, dass bei konditional — genetischer Betrachtung allen Gesetzen der einen Sinnesart solche der anderen entsprechen, und stützt sich dabei nicht nur auf die grosse Zahl der bekannten normalen und pathologischen Phänomene, sondern auch auf neue eigene Versuche. Aus der Gegenüberstellung erwächst die Überzeugung, dass die Motorik und Dynamik auf beiden Sinnesgebieten gleichartigen und entscheidenden Anteil an der Strukturierung des Raumes hat und dass dementsprechend auch die optische Raumwahrnehmung als ein Abtasten mit Sonden aufzufassen ist. Auch für die Metrik des phänomenalen Raumes ergeben sich Ansätze.

# STUDIES IN THE PSYCHOLOGY OF NEEDS: OBSERVATIONS AND EXPERIMENTS ON THE SEXUAL NEED IN HENS

ÅSE GRUDA SKARD

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### I. INTRODUCTION. THE PROBLEM

A certain number of *needs* seem to be existing in all human beings. These needs can be inborn, as e.g. the need for food, or acquired by later experiences, as e.g. in some human beings the need for certain drugs. In most needs we find a physiological as well as a psychological element, sometimes one, sometimes the other being the stronger. D. Katz has, through his experiments and his investigations, pointed out by other means how both physiological and psychological elements are at work in hunger and appetite (theory of two components); he has especially indicated which of the psychological laws will be operative in determining the tension of the need, or the „food appetite”<sup>1)</sup>.

D. Katz presumes as a hypothesis that the laws formed in the need for food will be valid for other needs as well. This hypothesis is certainly not to be granted except within the limits put up by the special laws valid for each particular need. We know e.g. that thirst if not satisfied will continuously increase, not reaching its maximum until the organism succumbs, while the strength of (or pain of) hunger, if not satisfied, will be increasing for a certain period and then decreasing again. The law, pointed out by Katz, that the appetite will be correspondingly stronger after a longer period of starvation, will then be valid only for this first period of lack of food.

In order to investigate whether the hypothesis of parallel laws for the different needs holds good, D. Katz started investigations of the sexual need in hens parallel to the experiments done on the

<sup>1)</sup> D. Katz: a) Hunger und Appetit. Leipzig 1932. b) Zur Grundlegung einer Bedürfnispsychologie. Acta Psychologica. Vol. II. 1935. The Hague. Martinus Nijhoff.

need for food. To evaluate the results of these observations, however, we shall have to consider the special laws valid for the sexuality.

The sexual need has, in most beings, its own rhythm, as is well known by everybody. In most animals we find special periods of heat outside which the sexual need does not seem to exist. We cannot expect any law found for the sexual need to be operative when the need is not existent, i.e. outside the periods of heat. Most animals have only one period of heat once a year, while animals such as dogs and cats have short periods of heat 2-4 times a year. In other beings we find a more constant readiness for sexual relations, at least for a certain part of the year, and in domesticated animals the periodicity may be more or less extinguished<sup>1)</sup>. Constant readiness we find e.g. in human beings and in apes, partly also in e.g. hens (at least cocks) which seem to be in constant readiness during the summer, but whose sexual relations come to an end during the winter<sup>2)</sup>.

In this fact we find one reason for choosing hens as objects of an investigation into the laws of sexual need. As the need is present during some length of time it is possible to make experiments, to vary conditions without running the risk that the need will disappear in the meantime. Another advantage of hens is that cocks are very active, — copulations numbering several dozens daily — so that we can rather easily study the influence of special conditions.

We may hope, in choosing objects where the sexual preparedness is lasting for some time, to find conditions which may be comparable with the bottom layers of the human mind. But, as there are in human beings large superstructures of intellect, emotions, traditions, moral notions etc. we cannot expect the underlying primary needs and the laws connected with them, to be as clearly exposed as they are in lower creatures.

The purpose of these investigations is to examine the special circumstances of sexual behaviour in hens and the laws respecting their sexual needs as well as their sexual appetites. As the time at our disposal for this investigation has been very limited, we

<sup>1)</sup> Meisenheimer, J. *Geschlecht und Geschlechter im Tierreich*. Jena 1921.

<sup>2)</sup> As Cook has observed in Eskimos (and in his own companions while living with the Eskimos') human beings under certain (arctic) conditions show a parallel absence of sexual need during the winter. Ll. J. Llewellyn. *Light and Sexual periodicity*. *Nature*, Vol. 129, 1932, p. 868.

do not pretend that we have attained our aim. The only thing we could possibly hope to do is to obtain a general survey of the prevailing *tendencies* which will have to be studied more thoroughly by further experimentation and observation with hens and other fowls.

## II. THE CIRCUMSTANCES OF THE OBSERVATIONS AND THE ARRANGEMENT OF THE EXPERIMENT

Our observations for this problem were carried out in Manchester during the May and June 1935. We had at our disposal a family consisting of one cock and 13 hens all hatched together in the beginning of August 1934, being thus at the time 10-11 months old. The hens were all of heavy breed, the cock a Sussex, the hens light Rhode-Islands. In addition to this family we had for special experiments 13 other hens, all belonging to another family, consisting of nine white Wyandottes and four dark Rhode Islands (all heavy breed) of about the same age as our first family. We also had another cock of the same age, of dark Rhode-Island breed, but not belonging to any of our hen families.

The place for observation was the original hen-run of our first hen family. This hen-run was 46 feet long and 39 feet broad. In one corner there was a hen-house consisting of two rooms, one (called the „hencoat”) 10 feet long and 8 feet broad, where the hens usually stayed when it was raining and where they also got much of their food, the other (called the „nest room”) 8 feet long and 4 feet broad, where were placed the three nests for egg-laying, as well as the perches. In the nest-room there was a box with its opening fitting to an opening in the wall leading out to the hen-run. The cock was placed in this box (the „dark room”) during the night, the opening then being closed. If this box was removed the opening served as an entrance from the hen-run to the nest-room. If not, the only entrance to the nest-room was through a doorway from the hencoat, and this doorway could be closed. In the hen-run was placed an isolation pen, 5 feet  $\times$  4 feet, made of wire-netting, a roof of sackcloth, having only a small opening (usually closed) on the side facing the hen-run. By means of a „tunnel” made of wire-netting this opening could be connected with the opening of the hencoat. This „tunnel” was used as well to conduct the cock from the dark-room to the isolation pen. For

the further transport of cocks or hens we had a little cage which could be opened on both sides. When the hens were not in the hen-run, they usually (Exp. 3 and 4) were placed in the adjoining run to which there was no door, the fence being made of wood (lower half), and wire-netting (higher half). In other experiments (6, 8) the hens or the cock were placed in the hen-run where the second family used to stay, separated from our run by two wirenetting fences in between which there was a footpath, 5 feet broad.

Our observation period consisted of 34 observation days. Of these the first seven days were given to observation of the hen family under its usual daily life conditions. Our only change in these was to separate the cock from the hens at night, i.e. from 9 o'clock in the evening until 9.30 in the morning. The hens were watched from about 9 o'clock in the morning, the cock from 9.30, all until 9 o'clock at night. This time at night was generally the usual time for the hens to go to sleep, sometimes they started even earlier, sometimes they continued going around a short time after nine. The time for getting up in the morning seemed to be around 4 o'clock. But as we could not possibly watch the hens at all times, we had the cock isolated to prevent any sex outlet while he was not observed.

After seven days the observation period was given to experiments with normal days interpolated. During the whole time the cock was put into the dark room at 9 o'clock at night, led from dark room to isolation pen at 9.30 in the morning, fed there until 9.40 and then let out into the hen-run. Exception from this only occurred as part of experiments (Exp. 1, 2 and 9). When the second cock was brought into the family, this routine was applied to him while, during this time, the first cock had his freedom in a neighbouring hen family. To save time later on we decided, after twelve days of observation, to omit the hours of the day observations which we had found to be least fruitful. Experience showed us that very little activity was going on in the middle of the day (see „daily rhythm“). We, therefore, dropped the observation from 11.40 till 3.30 putting the cock in the isolation pen for that time.

During the whole time we first observed the *sexual behaviour of the cock* and, as far as possible, the factors which seemed, in different ways, to be connected with this, the locality (in the hen-run, - coat, etc.), the choice of hens he seemed to have at

that time, what the hens were doing when he approached them etc. Then we noted the *sexual behaviour of the hen*. And in addition to this we tried to write down the *social behaviour of the hens* towards each other. Our notes on this point do not pretend to give any complete index of the social behaviour in our families, as we always had our attention centred on the cock. Nevertheless, our notes will give a fairly good impression of social behaviour. Besides this we made notes about the weather and the temperature. We also noted any special events occurring during the observation period.

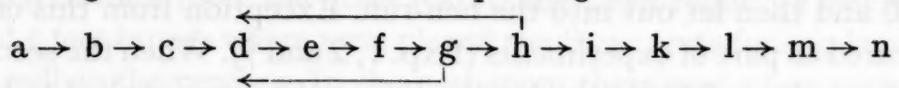
### III. GENERAL OBSERVATIONS

### A. Social behaviour

### a. General rules

Regarding the social order in a flock of hens Th. Schjelderup-Ebbe<sup>1)</sup> has pointed out the *pecking rule* which he has been able to confirm as a general rule in birds' societies. As to our family we can verify his findings on the whole. Some of the hens were very seldom pecked, and besides there seemed to exist a kind of constant peace between pairs of hens, so that it was rather difficult to point out the despot. But in most cases the pecking or chasing was repeated often and evidently enough to leave no doubt.

Among our thirteen hens the pecking order was as follows:



A hen would never peck back again immediately when pecked. She would never *answer* the pecking by pecking back, but *might* pass it on to some other hen. In our family no single fight occurred during the observation period. But it occasionally happened that a hen, who was usually the despot, one day was pecked by her „inferior”. This did not mean that she had lost her dominating position; the following days she was again the despot without any fight having taken place.

In this way e once pecked b, while b, on four different days, before and after had pecked e; h once pecked g, g pecking h on 12 different days, before and after. k on one day chased f diligently

<sup>1)</sup> Schjelderup-Ebbe, Th.: Beiträge zur Sozialpsychologie des Haushuhns. Z.f. Psychol. 88, 1922.

on two occasions; usually there was no pecking going on from f to k, but f had pecked k once before and generally seemed to be dominating. The position here, however, is a little doubtful. It may be that a kind of „triangle” existed in the pecking order, k being the despot of f, — as g and h are the despots of d —; we cannot exclude this possibility, though the general impression was not in favour of it. k also once pecked g, but here there is no doubt as to the real pecking order, as g pecked k on 11 different occasions, both before and after this incidence. The order of h and l is also uncertain: h has pecked l once, l has pecked h twice but both times on the same day; for the rest perfect peace seemed to reign between these two hens, but here again the general impression seems to be in favour of h as dominating.

The second family (of 4 dark Rhode-Islands and 9 white Wyandottes) was observed alone for very short time (3 days) so that it would be impossible to state anything definitely in this case. The pecking order there seemed to be:

A (dark RI) → D → M (dark RI) → L (dark RI) → E → B → I  
 → C → N (dark RI) → F → K → G → H

When the two families were mixed, the first family evidently was dominant over the whole second family, with the only exception that N pecked n. The reason for this absolute domination by one family may be that the gathering place was the territory belonging to the first family. According to the investigations by Schjelderup-Ebbe<sup>1)</sup> this was to be expected.

#### b. Individual differences

Our individual birds seemed very much alike even when we came to know them well. They were almost of the same size, also about the same colour etc. Only an expert eye was able to distinguish some small differences. Nearly all the hens were tight and even in the feathers. An exception was the hen g whose back was quite bare owing to the other hens’ and the cock’s tendency to eat her feathers. There was also a little moulting going on but not enough to any damage to the appearance of the hens. In fact only the hen h was clearly moulting.

The hens placed lowest on the pecking list were not smaller,

<sup>1)</sup> Schjelderup-Ebbe, Th.: Op. cit. p. 6.

nor thinner, nor less well feathered than the others <sup>1)</sup>). As a matter of fact one of the *smallest* hens was the dominant hen a. The feathers on a's head also seemed to be slightly shabby. But the lowest hen could not be distinguished as such by any apparent sign. Schjelderup-Ebbe maintains that the pecking order has a definite influence on the *appearance* of the individual. This may be true *generally*, but it was not confirmed in our family.

The hens' behaviour towards the observer did not seem to depend on their position in the pecking list either <sup>2)</sup>). Certainly both m and n were so shy as hardly to approach the observer except when very hungry and food was being provided by the observer. But this was even more the case with a and b who would not approach even when hungry. The tamest of the hens was c who would not only eat out of the observer's hand, but even jump up to sit in the lap and let herself be stroked and fondled. h was also tame enough to sit beside the observer on the bench. d, e and i would also eat from the observer's hand. The rest of the hens would never approach so near, but might be willing to have their food e.g. at the bench or elsewhere near the observer. Besides a and b, g and k were rather shy.

When *food* was presented the hens who had recently been broody were apt to be very eager to get it. At the beginning of the observations h was just „coming off the brood”. Later on during the observations i and n were in the same situation, and during the last days m was finishing her brooding time, all then giving evidences of great hunger and appetite. Otherwise c and e seemed to be the most eager to get food. b and k usually were the last ones to come to the food, sometimes they did not come at all. a was never first nor last; if there was no room for her when arriving at the food box she never chased any of the others away but went quietly around waiting until some one left. Once arrived at the food she did not suffer any interruption, but pecked hard any who interfered with her eating. About the same thing happened to h towards the end of the period: During the first time she was very patient, but in the last eleven days she seemed to be so exitable that she would not allow any other hen to eat from the box at the same time as herself.

<sup>1)</sup> Not in conformity with Th. Schjelderup-Ebbe, *Zur Sozialpsychologie der Vögel*. Z.f. *Psychologie*, Vol. 95, 1924.

<sup>2)</sup> Cf. Th. Schjelderup-Ebbe, *Weitere Beiträge zur Sozial- und Individualpsychologie des Haushuhns*. Z.f. *Psychologie*, Vol. 92, 1923.

*Intelligence* seemed to vary to a great extent among the hens of our family, the manifestations being, however, not clear enough to be ranged. When it came to finding or learning new ways (exits and entrances) the cock was obviously much slower and less ingenious than any of the hens. The hen k also seemed to be slow on this point, but this might be due to her lack of courage; on many occasions she proved less courageous than the rest of the family, this also keeping her from trying new outlets or relying upon them <sup>1)</sup>. The hens seemed to vary as to *sociability* as well. b and k were likely to avoid company, walking about mostly alone, each of them separated from any other fowl. d was not much attached to the others either; during the night she was always sitting on a separate perch all by herself.

The *aggressiveness* varies very much in the individual hens. The different number of peckings observed in the different hens will give some idea of this aggressiveness. As the observation of pecking was only of secondary interest in our work the number of peckings recorded will not be complete, but we may presuppose that the number observed will be approximately proportional to the real number. Hen i was by far the most aggressive; in spite of her being broody during the 10 first days of observation, which gave her but little opportunity to peck at that time, she was observed pecking 173 times up to the end of the observation period, counted in her own family only. If we count the number of peckings observed for each hen in her own family during the whole period, we obtain the following order:

i — 173, c — 135, b — 107, a — 102, f — 86, g — 82, d — 80, k — 50, e — 48, h — 31, l — 25, m — 6, n — 0.

Here we must consider that m was broody from the 6th day till the end of the period, staying most of her time lonely in the nest. If this had not been the case the amount of her peckings would certainly have been much higher.

Some of the hens seemed to change rather much as to amount of pecking during the period of our observation. While b, during the first 15 days of our observing her, only pecked 33 times, she pecked 74 times during the last 14 days. c and h had the opposite development; while c during the first 15 days pecked 111 times she pecked during the last 14 days only 24 times; the correspondent

<sup>1)</sup> Cf. D. Katz u. A. Toll: Die Messung von Charakter- und Begabungsunterschieden bei Tieren (Versuche mit Hühnern). Z. f. Psychologie, Vol. 93, 1923.

numbers for h are 22 and 9. All hens seemed to have days when their temper was especially bad or when one particular of their inferiors seemed to irritate them more than usual.

As a conclusion we can say that the total amount of peckings for each hen in our family seems not to depend on her place in the pecking order.

If we consider the amount of pecking in relation to the number of inferiors which each hen was allowed to peck, i.e. the number of times she has on the average pecked each of her inferiors, we get the following order: i — 43,3, k — 16,7, c — 13,5, f — 12,3, g — 11,7, d — 11,4 b — 9,7, a — 8,5, 1 — 8,3, e — 6, (m — 6)<sup>1</sup>), h — 5,2.

This order does not correlate with the pecking order, neither positively nor negatively. The pecking done by each hen in our family seemed to have nothing to do with her rank in the social order (corr. — 0.15).

The hens seemed to differ also as to how much they would irritate the others, i.e. as to how much pecking they were obliged to receive from their despots. Here the order would be: n received 238 peckings, m — 148 (in spite of her nearly constant absence!), i — 102, 1 — 98, h — 67, g — 65, k — 60, f — 43, e — 43, d — 37, c — 17, b — 6. This order is, as was to be expected, highly negatively correlated (— 0.94) with the pecking order. The lower a hen was on the pecking list the more pecking would she get.

One might think that the amount of pecking given by one hen would depend on the amount of pecking she received. Schjelderup-Ebbe says: „It is a general rule that a bird which is despot over only one or a few in its flock is very often a more strict despot over this minority than is a bird which is a despot over many”<sup>2</sup>. This seems in our family to be the case in one instance: i is a frequent pecker, receiving at the same time a large number of peckings. But on the whole the correlation between the two orders, the giving and the receiving of pecks, is slightly negative, — 0.43. The hens receiving much pecking usually will peck little themselves, and this in spite of the not very rare observation that one hen, when pecked, immediately passes the peck on to a third hen. This reminds us of certain

<sup>1</sup>) Broody most time.

<sup>2</sup>) Schjelderup-Ebbe, Th.: Social behavior of birds. A Handbook of Social Psychology, Worcester, Mass, 1935. Cf. also op. cit. p. 6.

observations made in dogs tied in file to a sled. If one of the dogs is hit by the driver, he often bites the dog going ahead of him.

If we here again take the average of the pecking of each hen by each of her despots, we get the following result: n — 19.8, m — 13.5, g — 13, i — 12.8, h — 11.2, e — 10.8, l — 9.8, f — 8.6, c — 8.5, d — 7.4, k — 6.7, b — 6. The pecking received from each despot does not seem to depend on her rank in the social order.

The amount of pecking from one hen to another seems to depend on the personal relationship between the two hens, on sympathies and antipathies<sup>1)</sup>. In some cases the despot will rarely peck the other, in other cases one or two hens seem to be particularly irritating to the despot. The first was so much the case in the relationship h — i that we were very doubtful as to which was the despot. A similar peaceful relation was found in a — f, a — k, f — k, h — d, k — l. While on the other hand a seemed to be particularly irritated by g and n, b by n, c by m and i, f by g and n, h by i, i by n, k by m, l by n.

Twice during our experiments we had just two hens in the hen-run for the whole day, once i and n (p. 214 f), another time a and g (p. 218 f). Under these conditions there was no pecking whatsoever going on when i and n were left alone together. But when a and g were isolated, a was observed pecking g 4 times.

\* \* \*

In the second family we had no occasion to observe the details of the social order, nor of getting much impression of the individualities of the hens.

One of the hens in this family, however, was so different from the others that one is tempted to call her „insane”. It was the hen N of dark Rhode-Island breed. Of appearance she was definitely smaller than the other R-I’s. She was very greedy and always rushed at the food. But her behaviour was strange, especially towards the other hens and the cock. She was the only hen observed who would occasionally peck back again immediately when pecked. This happened very rarely though. But she was cruel to her inferiors, especially chasing G whenever she had an opportunity to do so, e.g. always from the food, so that G hardly

<sup>1)</sup> Schjelderup-Ebbe, Th.: Op. cit. p. 6 and op. cit. p. 8 (note 2).

got any of the usual food provided for the hens. The most remarkable feature in N was, however, that she very often pecked the cock and even fought with him beak to beak in the manner of a fighting cock. Though she was much smaller than the cock she used to stretch up to peck his comb. He often ignored her or just pushed her away by force of his greater height. But sometimes he seized her by the comb to shake her very hard. Their actual fighting often was so cocklike that the observer first had the impression that N was a young cock and not a hen at all, until it was stated that N laid her egg every day and even was a more reliable egglayer than any other hen in her family. This, of course, does not exclude the hypothesis of disturbances in the internal secretion. — This extraordinary behaviour on N's part towards the cock did not interfere with their sexual relationship to which we shall return later on.

As to the rest of the second family we can only remark that the other three Rhode-Islands were bigger than the Wyandottes. Besides, K was a little smaller than the others and also caught the attention by her absolutely bare head. M was extraordinarily quiet and used to go about in seclusion. F and G were very peaceful, scarcely doing any pecking.

\* \* \*

In one of our experiments (Exp. 5 p. 219) A and B were placed together with our usual (first) family, in another the two families were mixed. (Exp. 7, p. 213). Both experiments gave interesting information of special individual features in our first family.

In the first experiment both A (brown) and B (white) were badly treated by the first family<sup>1)</sup>. They were not allowed to take any of the food while any of the other fowls were eating. They were not allowed to come into the hen-coat when it was raining and a separate room had to be prepared for them. But A was allowed to lie down to sleep beside the hens when they were sleeping outside the house, while B was excluded from this privilege as well. During the day A was pecked 66 times, B 93<sup>2)</sup>. This pecking is distributed on our hens in following way:

<sup>1)</sup> In accordance with Schjelderup-Ebbe, Th.: Fortsatte biologiske iakttagelser over Gallus Domesticus, Nyt Magazin for Natur-Videnskaberne, Bd 60, 1922. Cf. also op. cit. p. 8 (note 2).

<sup>2)</sup> As later was found, A was higher on the pecking list than B in their family.

	A pecked	B pecked	Sum
by a . . . . . . . . . .	1	1	2
b . . . . . . . . . .	2	1	3
c . . . . . . . . . .	6	16	22
d . . . . . . . . . .	0	3	3
e . . . . . . . . . .	0	1	1
f . . . . . . . . . .	2	5	7
g . . . . . . . . . .	5	9	14
h . . . . . . . . . .	10	9	19
i . . . . . . . . . .	5	9	14
k . . . . . . . . . .	8	3	11
l . . . . . . . . . .	2	11	13
(m) . . . . . . . . . .	25	25	50
n . . . . . . . . . .			

A remarkable thing to notice is that n, who has generally no opportunity to peck, now is doing nearly  $\frac{1}{3}$  of all the pecking, more than the double amount of any other hen <sup>1)</sup>. Disregarding c who is especially hard on B, all the larger numbers of pecking are shown by the hens low in the pecking list. e, a, b and d seem to be more tolerant towards the intruders. But, as a matter of fact, f was the first to go near them without touching them and the first to let them approach the food and the house. During this day A and B showed no sign of pecking each other, and the pecking within the old family was considerably less than usual.

The mixing of the two families gave as a result, as already mentioned, the obvious domination of the hens of the old family, with the single exception that the „insane” N pecked n. This experiment also gave some points of interest to the characterisation of our first hens.

We got the following distribution of peckings done by the old family on the new:

a pecked	once	on 1 hen (N)
b	”	” 1 ” (N)
c	”	7 times ” 5 hens
d	”	0 ” ” 0 ”
e	”	2 ” ” 1 ”

<sup>1)</sup> As also shown by Th. Schjelderup-Ebbe, op. cit. p. 12 (note 1).

f	pecked	2 times	on 2 hens	
g	"	13 "	" 8 "	(4 times on G)
h	"	0 "	" 0 "	
i	"	10 "	" 6 "	(4 times on A)
k	"	15 "	" 7 "	(7 " " N)
l	"	12 "	" 8 "	
n	"	15 "	" 8 "	

(m was broody, therefore absent).

The lower a hen was on the original pecking list the more she seemed likely to peck the new hens (corr. — 0,65); but this also depended on her own temperament and mood, thus c and g seemed to be more aggressive to the new family than should be likely considering their rank in the order, d and especially h being much more peaceful than supposed to. The only hens in the old family who pecked the leading hen A in the new family, were i and l. Our lowest hen n did not peck nearly so much when the whole new family was present as she did when only two of them were intruders.

When the two families were mixed no pecking occurred from one hen to another in the old family and only to a small extent in the new family (4 times in all). This fact is also observed by Schjelderup-Ebbe<sup>1)</sup>.

### B. Sexual Behaviour

#### a. General attitudes

##### α. The cock

1. **Different types of sexual behaviour.** With our cock the sexual behaviour was of different types. His different ways of approaching the hens might be distinguished as belonging on the whole to six different kinds. These different forms of behaviour cannot be separated from each other by strict limits. One sometimes led to or accompanied the other; within each form there were very different degrees of strength which would make it difficult to mark them off in groups. The reclassification of the different forms of sexual behaviour has been made chiefly for simplification. But it has been occasioned also by the fact

<sup>1)</sup> Op. cit. p. 8 (note 2) and p. 10 (note 2).

that the behaviour of the cock in this respect was not very varied, but seemed rather stereotyped, in spite of the different degrees of strength and of the different ways being mingled. The following will give an explanation of the different terms used throughout this paper to describe the behaviour of the cock.

I. Weak sexual *impulse*. This consists chiefly in the cock's raising the feathers of his neck. This movement will always be found at copulation or at an attempt to copulate. But it often occurred without any more serious act accompanying it. The need did not seem to be strong enough to produce further activity. We were therefore led to consider this raising of feathers unaccompanied by further activity, as the weakest sign of sexual need. We also at times observed the cock standing close to a hen and looking as if he were going to attack her; another time he would lift one foot from the ground as if to mount on her, lastly we even noticed him following a hen in a way as to be slightly sexually aroused. But on no occasion did we dare to consider this as a real sexual impulse, which should have left too much to the interpretation of the observer; the same obvious behaviour (standing close to a hen, lifting his foot, following a hen) also occurred when there was evidently no sexual factor active (as at food time or when the cock was going to sleep).

II. *Dancing*. When the cock was dancing to a hen, he lowered his wings, seemed to be scratching the ground with his spurs, and moving around the hen in a greater or smaller part of a circle. He might dance not only to one single hen, but to a flock of hens in general. This, however, occurred very seldom. Most of his dancing obviously had one special hen for its object. The dancing was sometimes accompanied by copulation, this depending on the responding behaviour of the hen (see later). It also happened that the cock danced to the hen after having copulated with her. This seemed to be done when the sexual urge had been very strong as was seen by his behaviour as a whole. To avoid making the matter too complicated, the dancing was counted in all cases as *one* kind of sexual behaviour besides (not included in) the other.— Sometimes, not very often, the cock approached a hen who was at some distance, by running towards her, his wings lowered so as to sweep the ground. When reaching her he would copulate with her, or try to do so; but rather often his energy seemed to have evaporated when he reached her, especially if she had been

running ahead of him for some time. This impressive approach consequently was accompanied by no acting or at any rate only by a faint taking of the hen by the neck. As this behaviour was an approach without in itself involving any touching of the hen (though stronger in its character than the weak impulse), and as it usually occurred at the same time of the day <sup>1)</sup> as the dancing, it has been included under the term „dancing” for simplicity’s sake. It did not occur often enough to necessitate a paragraph of its own.

III. *The cock puts his foot on the back of the hen.* This does not need any description. It generally occurred when a hen was lying down while the cock was standing, but also when both were standing, and the hen perhaps eating. It might be accompanied by copulation or attempt to copulate, in that case it was *not* counted as a special action, because the cock under copulation *always* puts his feet on the back of the hen. When the action was limited to putting the foot on the back of the hen the reason might be either lack of further energy in the cock, or the hen’s slipping away without his following her.

IV. *The cock catches the hen by the neck.* This (or the former) is the usual beginning of copulation. During the act the cock always seizes the hen by her neck feathers with his beak. But this behaviour sometimes occurred alone giving the impression that the cock had not energy enough to continue, or his action was stopped at this point because of the hen’s behaviour. What could possibly be the reason of this behaviour has not been taken into account. When it is accompanied by copulation or an attempt at such, it is, of course, not specially noted.

V. *Attempt at copulation.* When behaviour III and IV occurred simultaneously this was considered as an attempt to copulate. But the attempt was often more vigorous, the cock jumping up on the hen, catching her by the neck, lowering her wings by his feet etc. — i.e. making every effort from the weakest (mentioned first here) to the strongest, not resulting, however, in copulation. The reason for not succeeding might be lack of energy in the cock or resistance in the hen, which he did not manage to overcome.

VI. *Copulation.* During the act the cock is sitting on the hen’s back, seizing her by the neck, his feet pushing her wings rhythmically down so that her back part is turned upward, his own

<sup>1)</sup> See „daily rhythm”, p. 192 f.

downward. Their ani will meet by his lowering his tail and back part once or oftener, usually in rhythmic succession. The actual contact of ani lasts for a very short time, one to two seconds. Because of the surrounding feathers it is very difficult to see distinctly whether a copulation actually has taken place or not. Our notes on this point may, therefore, be slightly erroneous, though not so much as to be taken into consideration. On watching the hen from behind immediately after the act, we could usually see her anus open and the approximately identical inward-folding movements going on as after defaecation. We were not always able to observe this because of the hen's position in relation to the observer, and on these occasions we were obliged to suppose that a copulation had taken place when we saw the marked rhythmic movement of the cock's back part.

Beside these obviously sexual types of behaviour the cock also showed a sexual function as protector of the hens<sup>1)</sup>. As is well known, the cock will e.g. kick and scratch the ground to find food for the hens and he also on this point evinced a marked tendency to be chivalrous. He called the hens to the food very often before starting to eat himself and he would even offer them specially good pieces. He was observed several times picking a grain (of corn etc.) from the ground, holding it in his beak for a moment, clucking loudly, and dropping it again, pecking the ground beside it while calling loudly all the time until a hen came to snatch it. This even occurred one morning when he had hardly got any food the whole previous day (Exp. 10) while the hens had been well nourished all the time, having even had a good meal that same morning while the cock was still starving. But it should also be mentioned that he sometimes called the hens for some food but, on their running up to him, ate it all himself before they reached it. And it also occurred that the cock being himself hungry, went out into the middle of the hen-run calling the hens loudly to a place where there was no food at all. — Besides this the cock would gather the family around him calling out extremely loudly when some danger seemed to approach, as e.g. a seagull flying slowly across the yeard or a train whistling in the neighbourhood. He then occasionally chased them all into the hencoat being very impatient if some hen would not obey, and being himself usually the last to enter.

<sup>1)</sup> Th. Schjelderup-Ebbe, op. cit. p. 8 (note 2).

2. The daily rhythm. Different needs concerning the preservation of the organism, such as need for food and sleep quite evidently have a daily rhythm, partly based on the state of the organism, partly on habits (meal-times, bed-hour), other factors coming also into play<sup>1)</sup>. A similar daily rhythm in the

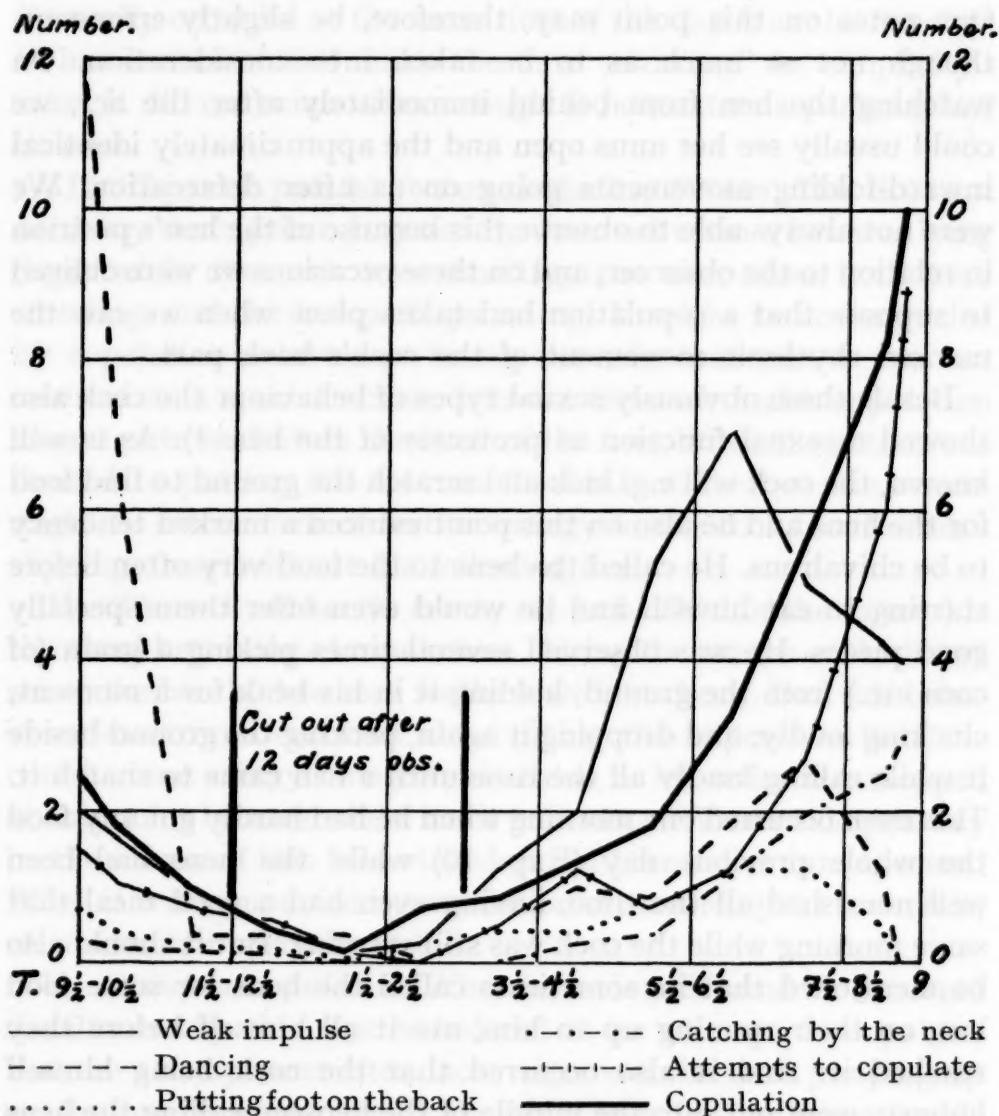


Fig. I. The sexual behaviour of the cock in average during a normal day. (The curves are smoothed except for the starting and the finishing point).

sexual need was also observed in the cock here studied. Fig. I gives a picture of this, representing the smoothed curves of the numbers of different sexual behaviour in a „normal day”, con-

<sup>1)</sup> Katz: Hunger und Appetit, Leipzig 1932.

structed by means of our observations during the first seven days. The following table gives the average of each kind of behaviour during each hour observed.

	9 $\frac{1}{2}$ -10 $\frac{1}{2}$	10 $\frac{1}{2}$ -11 $\frac{1}{2}$	11 $\frac{1}{2}$ -12 $\frac{1}{2}$	12 $\frac{1}{2}$ -1 $\frac{1}{2}$	1 $\frac{1}{2}$ -2 $\frac{1}{2}$	2 $\frac{1}{2}$ -3 $\frac{1}{2}$	3 $\frac{1}{2}$ -4 $\frac{1}{2}$	4 $\frac{1}{2}$ -5 $\frac{1}{2}$	5 $\frac{1}{2}$ -6 $\frac{1}{2}$	6 $\frac{1}{2}$ -7 $\frac{1}{2}$	7 $\frac{1}{2}$ -8 $\frac{1}{2}$	8 $\frac{1}{2}$ -9
Weak impulse	2	1,3	0,5	0,30	0,5	2	2	8,7	5,5	4,5	4	
Dancing	11,7	0,7	0	0,70	0	1	1	0,73	2,50			
Putting foot on the back	0,7	0,1	0,30	0	0,5	0,3	0,51	1,51	0			
Catching by the neck	0,3	0,1	0,30	0	0	0	0,5	0,32	2	3		
Attempt to copulate	1,5	0,6	0,8	0,30	0	1	1,50	2,53	9			
Copulation	2,5	1,1	0,80	0,51	0,72	1,75	6,5	10				

The main features in this finding are that the dancing is chiefly done in the morning, soon after the cock's joining the hens, while the copulation, the attempt to copulate and the catching by the neck decidedly increase towards the end of the day. The middle of the day is evidently rather dull, a fact which decided us later on to cut out our observations at that time. The daily rhythm described was found during our days of experiments as well.

The amount of the object of the cock's sexual activity at one time of the day did not seem to have much bearing on his activity at other times. Thus, much dancing in the morning does not necessitate a great number of copulations in the evening (the correlation for the 20 days where we have the numbers of dancing from 9,40-11,40 and the numbers of copulations from 6,30-9,00, is + 0,24). Nor does dancing for a certain hen in the morning seem to be an introduction to more serious sexual activity in the evening, as he will then very often copulate with hens to which he has *not* danced, and vice versa. The whole matter seems to be an appetite of the moment without consequences and continuity.

### β. The hens

1. **Types of behaviour.** As the hens obviously are not very active<sup>1)</sup> in the sexual relationships it is much more

<sup>1)</sup> Quite contrary to e.g. the female apes. Cf. Imre Hermann, Zum Triebleben der Primaten. *Imago*, Vol. 19, 1933.

difficult to classify or even to observe what might be called their sexual behaviour. On the whole the hens usually seem to be indifferent. The behaviour of the hens towards the cock as to sexuality might be described in the following terms:

I. The hen sometimes *invited* the cock to copulation. When he approached, either occasionally or with a *weak* sexual impulse (raising his feathers or dancing), but obviously without any intention of copulating, the hen might *crouch*, taking the same position as during copulation. This position must not be confused with a similar, though not identical, crouching which the hen sometimes made in order to slip away from the cock; the difference is, that when the hen had the intention of slipping away her forepart was slightly higher, her backpart more lowered, while when she was inviting the cock the forepart of the body was slightly lower than the back-part. This inviting crouching was, when it occurred, *always* noted, because it was rather easily observed. During the 29 days of the observation of members belonging to our first family it occurred 63 times in all, which means a little more than twice the day on an average.

II. This crouching also occurred as an *answer* to the cock's definite approach with the intention to copulate. In this paper we will use the word *invitation* for the hen's spontaneous crouching while the word *crouching* will be reserved for this behaviour when it occurs as responding to the cock's clear attack. In the latter case it was more difficult to observe being very often hidden by the cock's attack. Sometimes the hen was hidden by the cock so that the observer could not see her, sometimes it was impossible to distinguish whether the hen crouched or the cock forced her down. One must also bear in mind the occasional rapidity of the occurrence. — This crouching was observed in our first family 95 times, on an average, a little more than three times the day.

III. Very often the hen kept indifferent and *quiet*, apparently not being disturbed by the cock's approach at all whether this was just a weak impulse or an accomplished copulation. She continued the same kind of behaviour (eating, drinking, lying down, standing still etc.) not taking any notice of the cock be it in a positive or a negative direction. As this behaviour presents itself to the observer as a „non-behaviour” it has not been consistently noted throughout the observation period. It is,

however, noted 230 times, which means average nearly eight times the day.

IV. While keeping her body quiet, not showing any sign of resistance in this way, the hen might by different ways of *screaming* show some sign of resistance. She might *scream faintly* of which we are unable to say whether it is a sign of protest or the opposite. It occurred often under copulation or the cock's attempt to do so, but also sometimes when she slipped away from him. Usually this faint screaming occurred alone, without any other acitivity on the part of the hen. When it occurred, however, accompanying e.g. slipping away, it has been noted as a separate feature, as both of the actions most often occurred independently. In our notes this behaviour is found 120 times with an average of more than four times the day.

V. The screaming also might be quite ordinary, and we shall for this occurrence use the word *screaming* without any modification. This seemed to be an evident sign of protest though a very weak one. It usually occurred as a special feature, but might also accompany another action and was then counted by itself as the former faint screaming for the same reasons. It is noted 54 times, average about twice the day.

VI. The scream also might take the form of very loud *squawk* which was a stronger protest than the scream and often so loud that one would think the hen was being killed. However, this squawk was very often unaccompanied by any active resistance of the hen. In case of resistance it was usually noted separately. It is mentioned 42 times in our notes of the first family, nearly 1½ times per day on an average.

Of course there are no fixed limits between faint scream, scream and squawk. The screaming or squawking, however, always came in as a part of the behaviour when the hen was actually struggling to get free (see below), and as it then formed a necessary part of the whole activity, neither the screaming nor the squawking is specially mentioned on these occasions.

VII. The hen might show her unwillingness to indulge in sexual relationship in more active (and more effective) way than mere screaming. At the cock's approach she might quietly *walk* or *slip* away, *getting calmly up* when lying down or in another way get away from him very quietly. This is found in our first family 144 times, or 5 times the day on an average.

VIII. The hen also might *run* away from the cock, or *jump* to the side at his approach in order to avoid him. The difference between the walking and the running, of course, is mostly gradual, but not very difficult to distinguish. The running or jumping is noted 112 times, or on an average four times the day.

IX. Walking, slipping, running or jumping away were most likely to take place before the cock had managed to get hold of the hen. But they also occasionally were tried as a means of getting away even when the cock had taken hold of her. On these cases other forms of resistance were more usual. At first the hen might *wriggle*, *shake* or *twist* herself so as to get rid of the cock, she might take a long step forward and tear herself away from him or might move her body in circular fashion, her head being the centre kept quiet by the cock. This behaviour occasionally was accompanied by some kind of screaming. In our notes this shaking, tearing etc. was found 186 times, average nearly  $6\frac{1}{2}$  times per day.

X. If the resistance of the hen was still stronger we found her *struggling*, often very hard, to get free, combining most of the different movements mentioned in the last paragraph, using evidently all her force to get away and screaming or squawking at the same time. This was noted in our first family 51 times or nearly twice the day.

According to our notes the order of frequency for the different behaviour of the hens would be: 1. Keeping quiet (8 per day), 2. Shaking, tearing etc ( $6\frac{1}{2}$  per day), 3. Walking, slipping away, (5 per day), 4. Faint screaming (4 per day), 5. Running and jumping (4 per day), 6. Crouching (3 per day), 7. Inviting (2 per day), 8. Screaming (2 per day), 9. Struggling (2 per day), 10. Squawking ( $1\frac{1}{2}$  per day). Of these „keeping quiet” is the most likely to occur more often than actually noted.

Besides the response to the cock's sexual behaviour the hens also would respond to his behaviour in other ways: They would very often come when he called them to the food or to warn them against danger. Even the broody hens, who would avoid e.g. his dancing and were quite „non-sexual”, would respond in these cases if they were not on the nests.

2. The daily rhythm. After we had confirmed a certain daily rhythm in the cock (p. 192 f.) we could be pretty sure of

finding more samples of sexual (or resistive) behaviour in the hens as well as certain parts of the days. This also proved to be true: The average number of such behaviour decreases during the morning until a minimum in the middle of the day, and

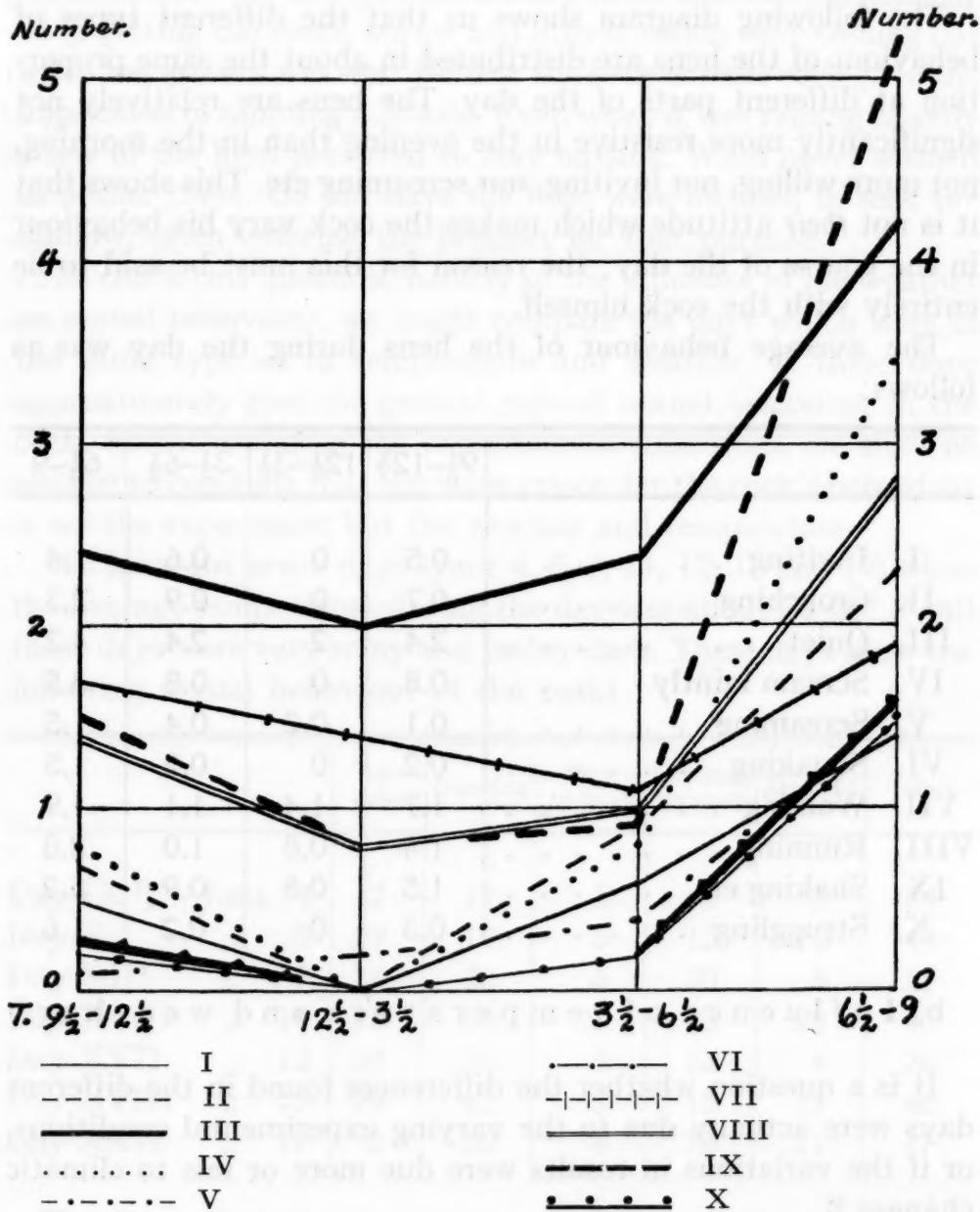


Fig. II. The hens' sexual behaviour in average during a normal day (table p. 198).

increases in the evening to reach its maximum towards the end of the day.

Bearing in mind that the cock is doing the greater part of his dancing in the morning and is copulating or trying to do so in the

evening, we might ask: Does this division depend on the different behaviour of the *hens* — they being e.g. more resistant in the morning, more receptive in the evening? Or has the daily rhythm got its basis in the cock?

The following diagram shows us that the different types of behaviour of the hens are distributed in about the same proportion at different parts of the day. The hens are relatively not significantly more resistive in the evening than in the morning, not more willing, nor inviting, nor screaming etc. This shows that it is not *their* attitude which makes the cock vary his behaviour in the course of the day; the reason for this must be said to lie entirely with the cock himself.

The average behaviour of the hens during the day was as follows:

	9½-12½	12½-3½	3½-6½	6½-9
I. Inviting . . . . .	0.5	0	0.6	1.4
II. Crouching . . . . .	0.7	0	0.9	2.3
III. Quiet . . . . .	2.4	2	2.4	4.2
IV. Scream faintly . . . . .	0.8	0	0.8	3.5
V. Screaming . . . . .	0.1	0.2	0.4	1.5
VI. Squaking . . . . .	0.2	0	0.2	1.5
VII. Walking . . . . .	1.7	1.4	1.1	1.9
VIII. Running . . . . .	1.4	0.8	1.0	2.8
IX. Shaking etc. . . . .	1.5	0.8	0.9	5.2
X. Struggling . . . . .	0.3	0	0.2	1.6

#### b. Influence of temperature and weather<sup>1)</sup>

It is a question whether the differences found in the different days were actually due to the varying experimental conditions, or if the variations in results were due more or less to climatic changes<sup>2)</sup>.

<sup>1)</sup> It is here a question of the influence of weather, temperature, light etc. only on the variations during the summer, not on the onset of the sexuality after the winter's absence of the need (this problem is discussed for various animals and plants in *Nature*, Vol. 129 and 130, 1932).

<sup>2)</sup> C. Ogle has shown how mating in white mice depends on their being in a humid, warm environment or in constant coolness, this last being the best for efficient sex functioning. (*Adaptation of Sexual Activity to Environmental Stimulation*. *American Journal of Physiology*, Vol. 107, 1934).

During the observation time we got the immediate impression that the hens, when it was raining heavily, would go into the hencoat being like to lie down there, while they would, when the rain was not very heavy, walk around in the hen-run particularly busy getting the small worms and insects which were coming out when the ground was wet. Neither the hens nor the cock gave the impression of minding a drizzle. Even when it was raining heavily a few of the hens preferred to stay outside. Wind never seemed to bother them. On hot days the hens were inclined to seek the shadow, often entering the hencoat for that purpose.

To check this question, namely of the influence of the weather on sexual behaviour, we might compare the days which were of the same type as to temperature and weather. If these days approximately give the general type of sexual behaviour in the cock, notwithstanding the experimental conditions, we shall be obliged to conclude that the *main* reason for the cock's behaviour is *not* the experiment but the weather and temperature.

We have got seven days (June 4, 5, 7, 11, 12, 13 and 19) when the average temperature during the day was about 50–55° F. All these days were very rainy and rather dark. These days show the following sexual behaviour of the cock:

		Im-pulse	Dancing	Foot on back	Catching by neck	Attempt	Copul.
Day IX <sup>1)</sup>	June 4	17	17	6	3	3	24
Day X	„ 5	29	19	5	13	12	44
Day XIII	„ 7	24	20	6	10	8	27
Day XVI	„ 11	25	18	27	23	23	43
Day XVII	„ 12	38	26	3	12	4	26
Day XVIII	„ 13	29	42	8	15	15	46
Day XXII	„ 19	3	26	4	13	11	17

The days when the temperature was around 55–60° F, were of two types: Three days in which there was rather much wind, and where cloudy and sunny weather was changing throughout the day. These days show the following behaviour:

<sup>1)</sup> The number given to the days does not depend on actual numbers of days we had at that time observed.

		I	D	F	C	Att.	Cop.
Day III	May 31	26	20	2	0	9	19
Day XII	June 3	33	23	8	20	14	53
Day XV	„ 10	35	32	2	8	4	36

There were also two days of this temperature which were rainy:

		I	D	F	C	Att.	Cop.
Day XI	June 2	15	13	9	38	21	13
Day XXIII	„ 20	9	10	5	5	6	23

Of the days when the temperature was around 60–65° F four were chiefly sunny days, two cloudy and rainy. The clear days gave the following result:

		I.	D	F	C	Att.	Cop.
	May 27	22	19	8	11	13	30
Day I	„ 29	16	23	8	7	5	39
Day II	„ 30	20	17	7	8	29	27
Day XIV	June 9	42	77	2	2	8	38

The cloudy and rainy days were the following:

		I	D	F	C	Att.	Cop.
	May 28	41	21	2	5	19	29
Day XXV	June 27	2	2	5	10	17	25

Days on which the temperature was 65–75° F were June 25, a sunny day, June 26 and 28, both cloudy. They gave the following result:

		I	D	F	C	Att.	Cop.
Day VIII	June 25	23	23	11	7	14	42
Day XXIV	„ 26	5	1	7	16	19	25
Day XXVI	„ 28	22	38	10	4	10	25

Still hotter days were June 24 and June 29, both sunny days with an average temperature of about 75–80° F.

These days gave:

		I	D	F	C	Att.	Cop.
Day VII	June 24	20	31	7	0	11	42
Day XXVII	„ 29	9	3	4	6	19	11

For the days May 20 to May 24 we are not able to give the whole sum of the cock's behaviour. On other days we were observing the other cock.

Our figures seem to be rather different for days of the same weather type, though we cannot assure that the weather has not *some* influence. We feel, however, justified to suppose that our experimental conditions play the greater role in the cock's behaviour.

### c. Individual differences

#### α. Cocks

1. Sexual activity. Besides our cock of Sussex breed we had the occasion to observe for five days (Exp. 8 and 9) a cock of the same age but of Rhode-Island breed.

There seemed to be remarkable differences between the two cocks as to sexual behaviour. The different forms of sexual behaviour were all found in the second cock except that this second cock rarely showed the weak impulse (by raising his neck feathers). But during the time we observed him he was much less vigorous than the first cock. On this point we shall have to take into account that the second cock was transported to a new territory and to a new family so that the psychological conditions were not the same for him. The influence of a new family will be discussed later (Exp. 6), but we are not able to give any facts as to the influence of a new territory on the sexual impulse. Still, the difference was so great that it can hardly be due merely to this fact, it also suggests some real individual difference.

On the three first days of observation of the second cock we found the following averages:

	9½-10½	11½	3½-4½	5½	6½	7½	8½	9 <sup>oo</sup>
Cop. . . . .	0	0.7	0.7	1	0.3	2	2	0.7
Att. . . . .	0	0	0.7	0	0	0	0.3	0
Catching b.n.	0	0	0.3	0	0	0	0.7	0.3
Foot on b. . .	0	0.7	0.7	0	0	0.7	0	0.3
Dancing . . .	5.3	3.3	3.7	5.3	4.3	5.7	0.7	0.3
Impulse . . .	0	0	0	0.3	0	0	0	0

Compared to the first cock's average day (p. 193) the figures are very small.

The daily rhythm also seems different (fig. III). The dancing has the same maximum in the morning, but it reaches a second

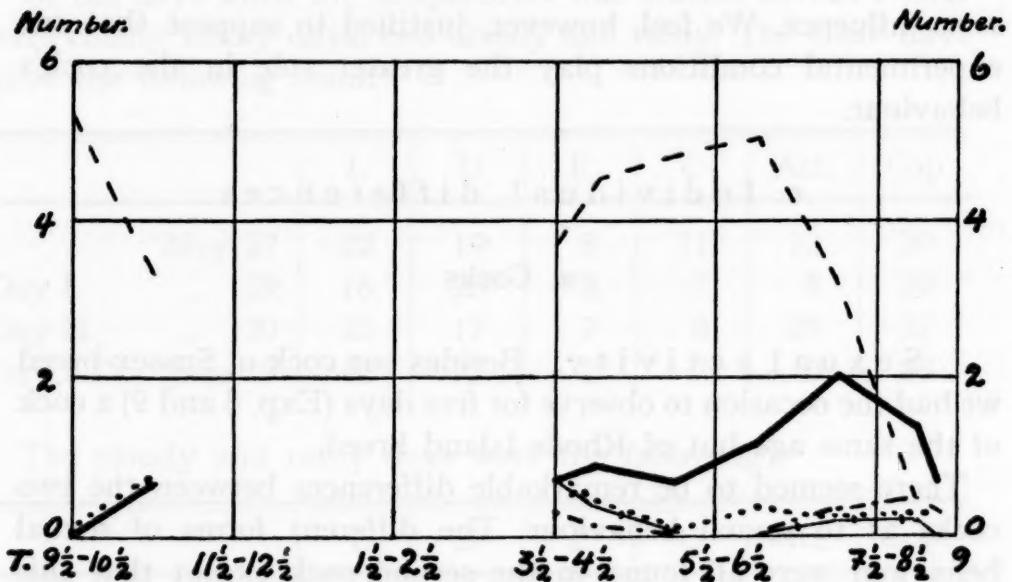


Fig. III. The sexual behaviour of the second cock in average during a normal day. (The curves are smoothed except for the starting and the finishing point. The signs are the same as in Fig. I).

maximum in the afternoon ( $6\frac{1}{2}$ - $7\frac{1}{2}$ ) where the number of dances is still higher. The maximum of copulations seem to be earlier in the afternoon in this cock than in the first. The other forms of sexual activity show such insignificant numbers that nothing can be said as to the rhythm regarding them.

## 2. Preferences among the hens.

aa. *The first cock.* Not all hens seemed to appeal to the cock to the same degree. We can, on the basis of our observations, make

up a preference list showing his taste. It was not, however, possible to decide *what* made one hen more attractive than another.

The cock would not touch a broody hen<sup>1)</sup>, but he might occasionally dance to her. When we started our observations, i and n were broody, m getting broody on our sixth day, so, of course, we have to exclude these three hens from our survey.

The cock had not the same hen as his favourite every day; but on the whole the same hens were usually in the upper part of the preference list. If we take the sum of all his approaches to the different hens during eleven days (up till exp. 6), we get the following preference list:

1. g (111 approaches), 2.a (103), 3.c (93), 4.b (77), 5.h (74), 6.e (67), 7.f (61), 8-9.d and 1 (both 46), 10.k (27). This gives a correlation with the pecking order of + 0.55. The cock was more likely to dance to g while the other top hens got a higher number of copulations.

If we take copulation, attempt to copulate etc. as more serious approaches than dancing etc., we are tempted to give higher values to this serious behaviour, and to evaluate differently the different kinds of sexual behaviour. This, however, will necessarily be based on purely subjective estimations; we may give e.g. 1 point to each „weak impulse”, 2 points to each „dancing”, 3 to each time he put his foot on the back of a hen etc. up till 6 points for each copulation. If this is done we get the following order:

1.a (407 points), 2.c (343), 3.g (313), 4.h (265), 5.b (260), 6.f (217), 7.e (213), 8.1 (181), 9.d (157), 10.k (97).

The difference between the two preference lists is not great (corr. = 0.94), the greatest being the place of g.

This second preference list gives a correlation with the pecking order of + 0.53.

As the first way of counting is the simpler, we shall use that in the following, especially as the two methods give approximately similar results.

During the eleven days we are considering here, we find the following distribution of the three highest places in the preference list:

<sup>1)</sup> Th. Schjelderup-Ebbe: Op. cit. p. 8 (note 2) and Psychologische Beobachtungen an Vögeln. Z. f. angewandte Psychologie. Vol. 35, 1930.

On the pref. list comes as	a	b	c	e	f	g	h
nr. 1	2 times	1 time	2 times	1½ times	1 time	4½ times	0 times
nr. 2	3 "	0 "	2 "	1½ "	0 "	2½ "	2 "
nr. 3	3 "	2 "	1 "	1 "	2 "	0 "	1 "

The other hens never reach any of these top places. And this shows that the same hens usually are high up on the list. This fact becomes still more evident when going through the whole list for all the days.

The low correlation with the pecking order tells us that the same qualities are not equally important for these two rank orders. It is impossible for a human eye to tell why the cock prefers one hen to another; beauty (from the human point of view), size, rank in pecking etc. do not seem to be essential. As we shall see below the "sex appeal" of each hen is not the same to different cocks either. Each cock has his favourites, but we cannot give any reason for his choice.

ab. *The second cock.* During the three ordinary days (Exp. 8, Day XIX, XX, XXI) we were observing the second cock it became evident that he also had his personal preferences among the hens.

His preference list is the following<sup>1)</sup>: 1.b (46 approaches), 2.i (17), 3.h (16), 4.a (11), 5. and 6.k and n (7 each), 7,8. and 9.e, f and g (6 each), 10.c (5), 11. and 12.d and 1 (1 each). This does not show any conformity with the first cock's preferences ( $c = +0.13$ ) from which we might conclude that the favourite hens of one cock is so more because of the cock's taste than of general attractive qualities.

### β. The hens

It is a question whether these favourite hens showed a more inviting behaviour towards the cock than the other hens.

During the same eleven days as used in chapter aa (p. 28) we find the following behaviour of the different hens (placed in their order of the first favourite list):

<sup>1)</sup> At this time i and n were off their brood.

	Inv.	Cr.	Qu.	F.s.	Sc.	Squ.	Go	Run	Shake	Str.	Sum
g	4	6	11	2	3	5	8	4	3	1	47
a	0	5	12	8	7	5	2	4	11	3	57
c	0	1	13	0	0	0	6	3	10	2	35
b	0	1	8	3	0	1	5	5	8	5	36
h	3	3	5	2	0	0	4	2	0	0	19
e	4	4	1	1	0	0	3	1	2	3	19
f	0	1	18	2	0	0	1	2	1	0	25
d	4	0	4	0	0	0	2	1	0	0	11
l	4	1	5	1	0	0	2	0	3	2	18
k	0	1	7	0	0	0	1	0	3	0	12

Here the total amount of sexual behaviour naturally will decrease as we go down the preference list, the cock's approaches being fewer. We get more „inviting” in the lower half of the list than in the upper (7-12), more neutral behaviour (Cr.-squ.) in the upper than in the lower (101-39) and much more avoiding behaviour in the upper than in the lower (86-27). The last two points are a natural consequence of the cock's more aggressive behaviour towards the upper group. But the figures certainly do not show that the preference list is established by reason of a more inviting attitude of the favourites.

When the new cock was introduced into the family, Day XIX, XX and XXI (the old cock being moved away), the hens would at first not obey his calling for food or warning of danger and they would constantly run away from his approaches whether they were sexual or merely occasional. Towards the end of the first day they seemed to get more used to him. The numbers for *running away* are:  $9\frac{1}{2}-11\frac{1}{2} : 9, 3\frac{1}{2}-6\frac{1}{2} : 8, 6\frac{1}{2}-9 : 1$ . The next day gave for the same hours: 0, 1, 0, the third: 0, 2, 0. The numbers for „*invitation*” for the three days were 1, 0, 0, for „*crouching*” they were 0, 2, 3: and for „*keeping quiet*” 0, 6, 6. These figures give an impression of the changing of the general attitude towards the new cock in course of time.

Then hens also showed individual differences towards each of the cocks. The table on this page gives for some hens an indication of their typical behaviour towards the first cock. We can note that g and a show a rather well balanced distribution of the different

kinds of behaviour (compared with their total amount of observed sexual behaviour and the total amount of each *kind* of sexual behaviour p. 193–196). c seems to be apt to try to shake or move in similar ways so as to get rid of the cock, b even to struggle to get free. h is comparatively inviting or quiet, as are also d and l, while e shows more fighting spirit beside the submissive attitude. f has as her typical behaviour to keep quiet, a trend which also is found in k though not so marked.

Besides this the following behaviour was observed: d had the habit of following the cock; very often she nearly seemed to be his shadow. b was usually the first to come when the cock called; she also was the first to obey the new cock and to approach on his call.

#### γ. The second family

During three days we observed the first cock in his old territory surrounded by the second family only.

In this family the cock definitely had his favourites as well. After the three days his preference list was this: 1. F (55), 2. D (49), 3. C (48), 4. N („the insane”) (46), 5. B (40), 6. I (36), 7. K (31), 8. H (23), 9. and 10. G and E (both 19), 11. M (12), 12. A (11), 13. L (8).<sup>1)</sup>

Of these hens N, M, A and L were brown Rhode-Islands, the rest White Wyandottes. This would indicate a preference for the Wyandottes. Besides the difference in colour there was here also a difference in size, the Wyandottes being smaller than the Rhode-Islands. Their sexual behaviour also differed (see later).

The cock very soon found this order; nothing but the result of the first day gives the following order: 1. and 2. D and F, 3. C, 4 and 5. I and K. 6. B, 7. G, 8 and 9. H and E, 10. N, 11. M, 12. A, 13 L. It is only N who has changed her place considerably.

In this case the correlation between the preference and the pecking order is + 0.24. This confirms the result of the observations of the first family, i.e. that there does not seem to be close connection between these two features.

The behaviour of the hens towards the cock in sexual respect

<sup>1)</sup> L was just finishing her broody period, so the first day she must still be recognized as broody. But even the other two days she was decidedly on the bottom of the list.

is rather different from the first family's attitude. During the observation is found in the three days:

„invitation” in all 31 times, or 10 times the day

„crouching”	22	„	7	„	”	”
„keeping quiet”	34	„	11	„	”	”
„faint scream”	8	„	3	„	”	”
„screaming”	8	„	3	„	”	”
„squawking”	5	„	2	„	”	”
„going away”	6	„	2	„	”	”
„running”	12	„	4	„	”	”
„shaking”	9	„	3	„	”	”
„struggling”	18	„	6	„	”	”

Compared to the numbers from the first family, this shows the second one as more submissive and especially inviting. The invitations, however, are mainly put together on the first day which gave 24, while the second showed 2, the third 5 invitations. „Keeping quiet” is equally distributed between the three days (11, 15 and 8). — The overwhelming numbers of „invitations” might be due to the fact that the cock was new to the hens. Compared to the behaviour of the first family towards a new cock, this behaviour is rather unexpected. Their different attitudes might be due to a special difference between these two families, or to the fact that the first family was in its own territory, the new cock coming to them, while the second family was transported on to the cock's territory, both the cock and the territory being in this way new to them. In the former case the cock did not know the territory, in the latter he did, which might possibly give another colour to *his* behaviour as well, thereby producing different response in the hens.

#### IV. OBSERVATIONS UNDER EXPERIMENTAL CONDITIONS

As already pointed out p. 177–178 the experiments to find out the special conditions affecting the sexual need cannot do more than indicate the tendencies which one might expect when continuing the investigations. The time at our disposal was too short to *repeat* the experiments in our family or to arrange parallel experiments in another family. For that reason we should hesitate to draw any general conclusions; what we found may be supposed

to be valid for other hen families as well, but we have no assurance that this is true. The rather short interval of time at our disposal also prevented our leaving many normal days in between the experimental days as we should have liked. We do not think that this reduction of normal intervals greatly affected the results of our different days, but we should certainly feel more assured if this arrangement could have been more satisfactory. The results also would have been clearer if we had had time enough to carry out more detailed experiments dealing with some of the special factors; some source of error might in that way have been excluded as e.g. the difficulty of varying *one* factor only at a time. As e.g. exp. 9 shows we were often obliged to arrange variations which implied in fact two or more changes at the same time. Though we tried to make up for these deficiencies, lack of time prevented us from doing this as thoroughly as we wished.

In the following the experiments are not treated in the order in which they were performed, nor are the numerals ascribed to the days in accordance with the actual order of the dates. But within any particular experiment, the order of the days will be indicated by the numerals ascribed to them.

### 1. *Storing of need — intensifying tension?*

From everyday experience and from the observations made by D. Katz<sup>1)</sup> we know that hunger increases during the first period of starvation so that absence of food for a certain time will lead to a greater intake when food is offered. To test whether the same held good in the case of sexual need we made a series of experiments and we also got informations on this point through experiments primarily designed to test other things.

In our first series of experiments (*Exp. I*) we did not let the cock out in the morning as usual, but kept him isolated, one day (Day I) until 12.00 noon, one day (Day II) until 3.00 p.m., and one day (Day III) until 6.00 p.m. We get the results for these days on the table page 209.

During these days the time in which the cock was able to act, was diminished by three hours from day to day. If this had no effect on the cock the sum of his sexual behaviour would decrease correspondingly. However, if he can store the sexual energy he

<sup>1)</sup> D. Katz: Op. cit. p. 18.

	Imp.	Danc.	Foot on b.	Catch by n.	Att.	Cop.
Day I	16	23	8	7	5	39
„ II	20	17	7	8	29	27
„ III	26	20	2	0	9	19
(Normal	29	21	6	8	20	31)

will use the rest of the day much more energetically so as to make up for the loss of time. On the whole this seems to be the case.

Though the proper „dancing time”, the morning, is cut out in all the three days, the number of dancings for the whole day has *not* decreased. The cock will do the same amount of dancing as usual when first he gets out. Most of this dancing is done during the first  $1\frac{1}{2}$  hours he is out, the numbers for this space of time being I 12, II 13, III 17<sup>1)</sup>. The sum of copulations is not very much changed, Day III only showing some decrease, perhaps owing to the very short time (three hours) which was left of the day. The only numbers which seem rather inconsistent are the number of *attempts*. The low numbers on Day I and III might be due to greater sexual tension in the cock so that he would not easily accept failure, but fulfilled his intention; or, they might be due to less resistance than usual in the hens caused by the longer absence of the cock. To settle the latter point we may look at the hens' behaviour during Days I and III. We find the following:

	Imp.	Crouch.	Quiet	F.scr.	Scr.	Squ.	Go	Run	Sh.	Str.
Day I	8	7	20	6	3	1	6	1	4	2
„ II	3	5	14	4	2	2	5	2	11	2
„ III	2	3	7	3	1	2	2	4	3	2
(Normal	2	3	8	4	2	$1\frac{1}{2}$	5	4	$6\frac{1}{2}$	2)

During Day I and Day II the inviting or submissive behaviour is definitely more frequent than normally but this is not the case for Day III. And as Day II shows an even *higher* number of „attempts” than normally (this difference, however, being small), we cannot consider the hens' behaviour as the only cause for the differing numbers of attempts. We may, however, consider it as

<sup>1)</sup> The higher number for Day III may be counted as a sum of the delayed „morning dances” and the usual „afternoon dances”.

one of the contributory factors. The full explanation of the varying number of attempts does not appear in our experiments. But there seems to be a storing of the sexual energy in the cock so that the total amount of sexual energy during the day will be constant and near to the normal.

In this series of experiments the feature of *dancing* was the most significant, as the „dancing time”, was the part of the day being excluded, the proper „copulation time”, the evening, being still intact. To see the effect of the cutting-out of the latter part of the day, for three days, Days IV, V and VI (*Exp. 2*), the cock was let out only from 9.40 to 12.10, being kept in the open-air isolation pen for the rest of the day. We afterwards studied the effect of these days for two days, Day VII and VIII. We get the following results:

		Imp.	Danc.	Foot on b.	Catch by n.	Att.	Cop.
Isol.	Day IV	0	27	10	0	6	7
after.	„ V	2	23	5	0	3	7
12.10	„ VI	8	29	3	0	6	6
Norm.	„ VII	20	31	7	0	11	42
days	„ VIII	23	23	11	7	14	42
	(Average)	29	21	6	8	20	31)

From Day IV to Day V to Day VI there is no significant change in the cock's behaviour. He does *not* in the morning Day V perform the copulations which he was *not* allowed to do the previous night. Neither does the Day VI show any increased activity. This indicates that there is probably *no* storing of energy over the night, from one day to the next. On the other hand the following *normal* days show a definite rise in the numbers of copulations, and from a closer study this turns out to be due to more frequent copulation during the morning hours. For the two hours in the morning these days show 11 and 9 copulations respectively (the average number for this time of the day being  $3\frac{1}{2}$ ).

In other experiments where for some reason the sexual activity was decreased during one day we find similar results: The *decrease* is usually followed by an *increase* the following day. We have e.g. *Exp. 3* and *4* (see p. 213, 214 and 218):

	Imp.	Danc.	Foot.	Catch	Att.	Cop.
Day IX (exp. day)	17	17	6	3	3	24
„ X (norm. day)	29	19	5	13	12	44
„ XI (exp. day)	15	13	9	38	21	13
„ XII (norm. day)	33	23	8	20	14	53

The numbers of copulations seem to be particularly significant in this case.

On the whole we can say that the sexual appetite when not satisfied to the usual degree at a certain time, tends to increase so that the deficit is compensated for by increased activity at the next opportunity. This law, however, is not so clear and definite in its application to the sexual need as it is to the need for food <sup>1)</sup>.

Observations from human life teach us that the same holds good to a certain extent there as well, where it is not dissimulated or restrained by other factors. Sailors coming to a harbour tend to be rather „wild” during the first days, the stored energy resulting in excessive activity. Further evidence is supplied by the behaviour of soldiers on leave, and prisoners immediately after release. It is also well known that in persons used to regular sexual outlets, abstinence will demand greater self control during its first period than later on when abstinent habits are established.

## 2. *The possibility of changing the daily rhythm*

Exp. 2 which has just been reported was also arranged to see whether a new habit would bring a change into the daily rhythm in the life of the cock. If for a series of days the cock had *all* his copulations in the morning, would this lead him to continue with the transference of his chief copulation time from the evening to the morning?

Unfortunately the time we had at our disposal for this experiment was rather short, not providing more than three days for exercising the new habit. The result for the three days and the following two normal days is given in the table p. 210. As we have already pointed out, during the three days (IV, V and VI) there

<sup>1)</sup> This problem and this parallel is for human life touched on by Havelock Ellis, *Studies in the Psychology of Sex*, Philadelphia 1920, Vol. VI, p. 198f.

is no significant change. We shall analyze the days which followed a little closer. The different aspects of sexual behaviour show the following distribution as to the hours (compared to the average of normal days at the same time):

	9½-10½	-11½	3½-4½	-5½	-6½	-7½	-8½	-9°°	Sum
<i>Copulation:</i>									
Day VII	7	4	3	4	3	6	7	8	42
Day VIII	5	4	4	3	4	4	12	6	42
(Normal	2.5	1.1	0.7	2	1.7	5	6.5	10	31) <sup>1)</sup>
<i>Attempts:</i>									
Day VII	1	0	2	1	0	2	4	2	12
Day VIII	1	0	0	0	2	6	3	2	14
(Normal	1.5	0.6	1	1.5	0	2.5	3	9	20) <sup>1)</sup>
<i>Take by neck:</i>									
Day VII	0	0	0	0	0	0	0	0	0
Day VIII	2	0	0	0	0	1	3	1	7
(Normal	0.3	0.1	0	0.5	0.3	2	2	3	8) <sup>1)</sup>
<i>Foot on the back:</i>									
Day VII	2	0	1	0	0	1	3	0	7
Day VIII	1	2	3	0	0	3	2	0	11
(Normal	0.7	0.1	0.3	0.5	1	1.5	1	0	6) <sup>1)</sup>
<i>Dancing:</i>									
Day VII	12	0	5	5	0	4	3	2	31
Day VIII	13	0	1	0	3	3	2	1	23
(Normal	11.7	0.7	1	1	0.7	3	2.5	0	21) <sup>1)</sup>
<i>Weak impulse:</i>									
Day VII	2	0	3	3	6	3	3	0	20
Day VIII	6	1	7	4	3	0	2	0	23
(Normal	2	1.3	2	2	8.7	5.5	4.5	4	29) <sup>1)</sup>

The features of interest in this table are the copulation and the dancing. In Day VII we find the extra numbers of *copulations* (to make the increase 31-42) during the *morning* hours. This is

<sup>1)</sup> This is the *correct* sum, found when no time was left out (see p. 19) and therefore a little higher than the sum of the averages here given.

not merely due to the release of energy stored from the previous days, other days which also gave outlet for a similar storing (Exp. 11 and 4) showing increase in the evening hours as well. On the other hand the additional *dancing* of Day VII is found chiefly in the *afternoon*. This indicates that the three days without any activity in the afternoon have had some influence on the distribution, the daily rhythm of copulation and dancing. But this weak effect does not last very long: Day VIII seem to manifest a return to the usual daily rhythm, nothing but an insignificant displacement being still noticeable.

This shows the possibility of changing the daily rhythm by exercising new habits. It is, however, a question whether such changes will ever be so well established that they will not be dropped again rather quickly if the cock is left to himself. — Parallel to this one may also ask whether an animal trained to certain hours for his meals will not also drop this habit if left to himself surrounded by opportunities for getting food <sup>1)</sup>.

### 3. *Influence of a great number of opportunities on the sexual appetite*

Regarding the appetite for food D. Katz has shown <sup>2)</sup>, that under normal conditions the appetite and the amount of food eaten will increase if the amount of offered food is augmented. Two experiments were carried out in order to see if this holds good also for sexual appetite.

The cock was left alone (*Exp. 3*) for one day (Day IX) with no more than two of his usual hens (in stead of thirteen). On another day (Day XVIII) he was surrounded by 26 hens (*Exp. 7*), all known to him beforehand (see p. 220 f). The days give the following numbers:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day IX	17	17	6	3	3	24
Day XVIII	29	42	8	15	15	46
(Normal	29	21	6	8	20	31)

<sup>1)</sup> This seems to be the case, judging from the studies by Gallus Beck: *Neue Beiträge zur Zweikomponententheorie des Hungers*. Z. f. Psychologie, Vol. 118, 1930.

<sup>2)</sup> D. Katz: Op. cit. p. 18. Also Erwin Bayer: *Beiträge zur Zweikomponententheorie des Hungers*. Z. f. Psychologie, Vol. 112, 1929.

The numbers for Day IX are nearly all *lower* than the normal, the numbers for Day XVIII nearly all *higher* than the normal. In this respect the numbers of copulations are especially significant.

This indicates the same law of sexual appetite as of appetite for food: The greater the number of opportunities presented (within certain limits), the greater the appetite.

Our experiment does not show, however, whether this increased activity would *last*, when the cock had been surrounded by the increased family for some length of time. It is possible that his activity would again drop to the usual amount when he got *used to* the greater number.

#### 4. *The demand for quality*

As regards the need for food and the appetite connected with this Katz points out <sup>1)</sup> that the concern about its taste and nutritive value decreases with increasing difficulties in obtaining food.

To find out whether this holds for the sexual appetite as well, we left the cock for one day (Day XI) alone with two broody hens (*Exp. 4*). As mentioned before (p. 203) a cock under usual conditions will never touch a broody hen. During Day XI the cock was not isolated in the middle of the day; he was allowed to move about freely with the two hens and was continuously observed.

Day XI shows the following development:

- 9½–10½ Dancing 3 times.
- 10½–11½ Weak impulse three times, dancing twice, putting foot on the back twice.
- 11½–12½ Weak impulse once, dancing four times, putting foot on the back once.
- 12½–1½ Putting foot on the back once.
- 1½–2½ Weak impulse once, dancing once.
- 2½–3½ Weak impulse once. At 3.21 first copulation <sup>2)</sup>.
- 3½–4½ Weak impulse once, putting foot on the back once, attempt twice, copulation 3 times.

<sup>1)</sup> D. Katz: *Op. cit.* p. 18.

<sup>2)</sup> It was extremely difficult to observe whether the broody hen really was able to have copulation. What is counted as copulation during this experiment *seemed* to be such, but the fact cannot be assured.

4½- 5½ Weak impulse once, dancing once, putting foot on the back once, attempt once.

5½- 6½ Weak impulse twice, dancing twice for the other hens who were in the neighbouring yard, putting foot on the back once, catching by the neck 3 times, copulation once.

6½- 7½ Weak impulse 3 times, catching by the neck 3 times, attempt 7 times, copulation 3 times.

7½- 8½ Weak impulse twice, putting foot on the back once, catching by neck 25 times, attempt 7 times, copulation 3 times <sup>1)</sup>.

8½- 9°° Putting foot on the back once, catching by the neck 7 times, attempt 4 times, copulation twice <sup>2)</sup>.

The cock's initial repugnance to the broody hens was overcome in the afternoon by his appetite, no other outlet having been found for his sexual need. After this it seemed to be easier for him to employ them later on. The totals for the day were:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XI	15	13	9	38	21	13
(Normal	29	21	6	8	20	31)

The most remarkable feature in this table is the catching the hens by the neck. Among the 38 times it occurred, 25 times are noted from 7½-8½, and among the latter 15 occurred in the course of six minutes, nothing else taking place during this time. While observing this we got the impression that the cock lacked energy to continue the act; he seemed to „change his mind”, or, in other words, the sexual need seemed not to be great enough to overcome his repugnance. Parallels to the food situation present themselves. A similar situation is met with e.g. when a hungry person in want of food is lifting a spoonful of disgusting soup to his mouth, but disgust overcoming the hunger, lowers it again in the decisive moment.

As to the cock's „non-sexual” behaviour during this day we noticed that he was comparatively restless and very irritable and more aggressive than usual towards the observer.

<sup>1)</sup> Among these two were somewhat questionable.

<sup>2)</sup> Of these one was doubtful.

The attitude of the broody hens was distinctly resistive towards the cock. They did not come when he called, and their sexual behaviour (very similar in both hens) is indicated by the following numbers for the two together:

	Inv.	Crouch	Quiet	F.scr.	Scr.	Squ.	Go	Run	Shake	Str.
Day XI	0	0	2	3	1	1	4	1	14	5

As is seen the numbers of active resistance are much higher than usual<sup>1)</sup>.

The *following day* (Day XII) was arranged as a normal day. The cock here compensated for the deficit of Day XI (see p. 211). An interesting feature to be noted, well known by all poultry holders, was that the two broody hens, i and n, were now „off their brood”, not giving sign of any disposition to go back to the nests again; at the same time they had just a little left of the characteristic clucking sound. The cock did not avoid them as during their broody period, but he did not pay much attention to them. They were placed lowest on his preference list for that day, though they did not remain there the following days (compare exp. 3).

In human life we very often find instances showing that the same law at bottom holds for the sexual need: When the best is not to be had, the second best will be taken as a substitute, the demands being lowered as the need is increasing (within certain limits). We think once more of the sailors being satisfied with unattractive girls when at liberty ashore, — white men in the colonies living with native women who at first aroused nothing but disgust, — women marrying men whom they have once blankly refused, when others are not to be had (here also economical, conventional and self-asserting factors are coming into play). When members of the opposite sex are not available, men will, as well as animals<sup>2)</sup> employ members of the same sex as a substitute. This is often the basis of homosexuality among prisoners<sup>3)</sup>, among soldiers in the barracks or during the war,

<sup>1)</sup> Th. Schjelderup-Ebbe in op. cit. p. 8 (note 2) notes that broody hens avoid copulation and that copulation is really painful for a hen in this state.

<sup>2)</sup> Meisenheimer, Op. cit. p. 3.

<sup>3)</sup> Cf. e. g. R. Plischke, Zur Frage der Sexualnot der Gefangenen. Arch. f. Kriminologie, Vol. 84, 1929. — W. Gentz, Das Sexualproblem im Strafvollzug. Z. f. die ges. Strafrechtswissenschaft, Vol. 50, 1930.

among sailors at sea, among pupils in boarding schools <sup>1)</sup> (often dissimulated, especially so in girls <sup>2)</sup>), etc. And when heterosexual connections are not available or are refused for some reason, both men and animals are known to satisfy their needs more or less frequently by substituting themselves for a partner (masturbation). Animals will generally resort more easily to homosexual relations than to masturbation <sup>3)</sup>. In men so many factors are at play on this point that we can hardly distinguish the fundamental trends.

This law of substitution is found in other needs as well. Thus Lewin and his pupils <sup>4)</sup> have proved its validity when a need was created for solving a problem or finishing a task. They also point out that the attaining of a substitute aim will not usually provide the same amount of satisfaction as the reaching of the original aim. This probably holds good for other needs too. A substitute generally is not able to replace entirely what was originally the object of our efforts (cf. also p. 224).

### 5. *Special over-satisfaction*

In the problem of „food-appetite” one substitute can rarely be satisfactory partly for the reason that some chemical component would usually be lacking. Experiments have shown <sup>5)</sup> that we sometimes outside of ordinary hunger feel a particular kind of hunger, the hunger for *one special stuff* even if other stuffs abound. Thus hens can show hunger for chalk though being well nourished in all other respects. Human beings can show special appetites for meat, for sweet stuffs etc., springing partly from chemical factors.

On the other hand overfeeding of a certain stuff will create oversatisfaction, abolishing the appetite for that kind of food for a certain period. Forced milk-drinking as a cure for illness

<sup>1)</sup> M. Hirschfeld, Die Homosexualität, chapter VIII. — Havelock Ellis, op. cit. p. 37, Vol. II, p. 86 and 325.

<sup>2)</sup> Havelock Ellis, Vol. II, p. 368 f.

<sup>3)</sup> Meisenheimer. Op. cit. p. 3. — Havelock Ellis, Vol. II, p. 4-8.

<sup>4)</sup> Wera Mahler, Ersatzhandlungen verschiedenen Realitätsgrades. Psychologische Forschung, Vol. 18, 1933. — Käte Lissner, Die Entspannung von Bedürfnissen durch Ersatzhandlungen. Psychologische Forschung, Vol. 18, 1933. — Sarah Sliosberg, Zur Dynamik des Ersatzes in Spiel- und Ernstsituationen. Psychologische Forschung, Vol. 19, 1934.

<sup>5)</sup> D. Katz: Op. cit. p. 18.

will lead to dislike for milk during the following period. Katz' experiments show that the same thing holds good in hens<sup>1)</sup>.

In order to investigate this problem in the domain of sex, we left the cock alone for one day (Day IX) with two of the hens namely two of his favourites g and a (*Exp. 3*), to study the effect worked upon the followings days. We got the following results:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day IX						
{ a	6	6	5	3	3	12
{ g	11	10	1	0	0	12
Day X						
{ a	0	0	1	0	2	2
{ g	2	0	0	0	0	5
Day XIII <sup>2)</sup>						
{ a	2	4	1	0	0	0
{ g	1	0	0	0	0	0

As shown p. 203–204 a and g had up till this experiment headed the preference list. On Day X the preference list is: 1.b, 2.n, 3–5.c, h and m (equal), 6.f, 7.k, 8–9.e and g (equal), 10–12.a, d and i (equal) [m was broody]. On Day XIII the preference list of these hens was: 1.f, 2–3.c and 1, 4–6.a, b and e, 7–8.h and k, 9.i, 10.n, 11.d, 12.g. During Day XIII the cock not once as much as *tried* to copulate with a or g. — After some days of absence from his own family the cock seemed partly to have regained his appetite for these two favourites though they never again during the observation period reached the former leading position in the preference list<sup>3)</sup>.

This experiment shows that oversatisfaction can also take place regarding sexual appetite.

It is evidently very difficult to find parallels to this in human life as just on this point the human relations are usually disguised by other overwhelming factors. The monogamous marriage where one might expect to find most of „over-satisfaction” is influenced

<sup>1)</sup> Erwin Bayer, op. cit. p. 39.

<sup>2)</sup> Between Day X and Day XIII there was one day without observation.

<sup>3)</sup> Regarding other consequences of this exp. see p. 211.

by many other elements besides mere sexuality — of erotic factors from other spiritual levels, of deeper emotional factors, intellectual and moral factors as well, and, not to be forgotten, considerations concerning the offspring come into play. In a marriage we never find merely sexual factors at play. But even so we very often get instances of spouses getting *sexually* tired of each other though the relationship itself has been satisfactory<sup>1)</sup>.

The purely sexual relationships found in human beings are usually of short duration, seldom leading to over-satisfaction. We have nevertheless not few instances of men not wanting to see again the prostitutes with whom they have had relations; but on this point many other factors are present as well (such as shame, fear of reputation).

#### 6. *Adding of a new stimulus*

A new dish on the table may give rise to different attitudes. A child often reacts strongly *against* a new dish. But frequently the very novelty of a dish will give to it a special attraction.

By adding two new hens, A and B, to the old family (*Exp. 5*) we expected to see something similar to the latter reaction. The experiences with oversatisfaction by two favourites made us presuppose the attractiveness of new stimuli.

The hens in the first family reacted strongly against the two new hens (p. 186). And so did the cock at first. While he would never chase or peck any of his own hens antagonistically, he chased A (brown Rh.-I) 24 times and pecked her 13 times; he chased B (white W.) 5 times and pecked her once.

During the whole day (Day XIII) we find that the cock only once put his foot on the back of A, that was all that could be observed in the way of sexual behaviour towards her. Towards B he was more sympathetic: not only did he show less antagonism (see above), but he also gave proofs of the following sexual behaviour:

Weak imp. 3, Dancing 5, Catching by neck 1, Attempt 1, Copulation 4.

But this positive behaviour only gradually manifested itself.

The behaviour of the new hens towards the cock gave nothing of importance. A once screamed when he approached and once

<sup>1)</sup> We cannot here discuss all the different factors in marriage.

went away. B once crouched, once kept quiet, once tried to shake him off.

The totals (reactions towards the old + the new hens) for this day (XIII) were not remarkably different from the average day:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XIII	24	20	6	10	8	27
(Norm.	29	21	6	8	20	31)

The increased number of hens this time did *not* increase the sexual activity in the cock. It is possible that some energy was dissipated in *fighting* the two new hens (compare Exp. 9 p. 222 f.). But this cannot be confirmed.

### 7. *Changing stimuli*

As already mentioned several times (see p. 178, 181, 206), the cock was for some days surrounded by a new family of the same size and age as his original family (Exp. 6). This experiment was first done on Day XIV and XV. Here the cock did not at all object to the new hens as was the case when the two hens were added to his old family. And in this case the novelty of the stimuli seemed to excite him to greater activity than usual, Day XIV giving higher numbers than Day XV when he had got more used to the second family.

The increase was not due to greater attractiveness of the second family, which was proved by his behaviour on Day XVI, when we gave him his old family back again; *this* family then having a touch of novelty, a considerable increase was undoubtedly observed.

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XIV (new fam.)	42	77	2	2	8	38
„ XV (new fam.)	35	32	2	8	4	36
„ XVI (old fam.)	25	18	27	23	23	43
„ XVII (new fam.)	38	26	3	12	4	26
(Normal	29	21	6	8	20	31)

This increase, however, was *not* repeated when he was again on Day XVII (the following day) surrounded by the second family. The slight decrease on Day XVII may be due to the great expenditure of energy during the three previous days (opposite to the storing of energy). The constant low number of „attempts” in the second family (Day XIV, XV and XVII) may also be due to the more submissive attitude of the hens in this family (see p. 206-207).

But the decrease Day XVII as well as the increase of Day XVI may also be due to the preference for his own family which is manifesting itself on Day XVIII when the two families were both present. The preferences demonstrated when both families were present were as follows (the preferences within each family are given p. 203 and p. 206): 1. F, 2-3.a and f, 4-5.b and i., 6.1, 7-9.c, g and C, 10-11 n and N, 12-14.e,k. and D. 15-17.d, L and E, 18-19. B and K, 20.h, 21-23. A, M and I, 24-25. G and H [m was broody]. Of the first 11 8 are of the first family, and of the last 11 9 are of the second family. — If, in this case we use the method of giving more points to the copulation than to the attempt etc., counting the weak impulse as the least valuable, we get the following order: 1.f, 2.a, 3-4.i and l, 5.g, 6.b, 7-8.c and C, 9.F, 10.k, 11.n, 12.e, 13.N, 14.D, 15.E, 16.B, 17.d, 18.h, 19-20.L and I, 21.K, 22.A, 23.M, 24-25. G and H. This shows still more clearly that the cock was more attracted by his first family than by the second.

As a summary of the effects of this experiment on the cock we might say that variations of the stimuli (such as in changing the families) tend to increase the sexual appetite. The same tendency was seen in the appetite for food <sup>1)</sup>. New stimuli seem to have the effect of arousing the need to a higher tension than usual.

#### 8. *Effect of competition*

Experiments with food <sup>2)</sup> show that *social factors* effect the appetite. A hen will eat more when she is surrounded by other eating hens than when alone, and this even to the extent that a hen satisfied will start eating again when a hungry hen is placed

<sup>1)</sup> Katz, Op. cit. p. 18.

<sup>2)</sup> D. Katz: Op. cit. p. 18. E. Bayer: Op. cit. p. 39. G. Beck: Op. cit. p. 39.

at the food supply and starts eating. Not only will the satisfied hen eat greedily anew, but she even sometimes will try to make the hungry hen keep away from the food, by chasing, pushing or pecking her (her doing so depends on the pecking order among the two hens in question).

Yerkes<sup>1)</sup> points out that the same incidents of suggestion are found among apes and monkeys also in the sexual need: at the sight of one individual behaving sexually, another will be induced to behave similarly.

Because of the well-known fact that two cocks placed in the same hen-run will fight, sometimes until one of them is killed (cf. cock-fights)<sup>2)</sup>, we were unable to obtain comparable results by placing a new cock in the same family as long as the first cock was present. In order to get the cocks as near to each other as possible without opportunity to fight each other fatally we made some special arrangements. During Day XIX, XX and XXI (*Exp. 8*) the second cock was placed with the first hen family on its territory, the first cock being temporarily placed with the second family. When Cock II was supposed to be sufficiently accustomed to the territory and to the hens, as well as the hens to him (see p. 205), for two days (Day XXII and XXIII) we divided the hen run into two approximately equal parts by means of wire netting, leaving half of the hencoat approachable from each part and closing the door between the rooms in the hencoat. We then left one half of the family on each side, and one cock on each side (*Exp. 9*), changing the cocks from Day XXII to Day XXIII so that each cock for one day had each half of the family. The two days gave the following numbers:

Cock I	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XXII	3	26	4	13	11	17
„ XXIII	9	10	5	5	6	23
(Normal	29	21	6	8	20	31)

We must here take into account that each of the cocks had only one half of the usual family; although the cocks saw the whole

<sup>1)</sup> Robert M. & Ada W. Yerkes: Social Behavior in Infrahuman Primates. Handbook of Social Psychology, Worcester, Mass. 1935.

<sup>2)</sup> Cf. Th. Schjelderup-Ebbe, op. cit. p. 8 (note 2).

Cock II	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XXII	0	35	1	0	1	4
„ XXIII	4	7	9	0	4	10
(Normal	0	29	2	1	1	7)

number of hens and even might dance to hens in the other part of the yard (and actually did so), each of them had not access to more than six hens at the time (m being still broody). This might (p. 213 f.) tend to lower the frequency of sexual behaviour. On the other hand Cock I had been absent from his family for three days, a fact that might (p. 220 f.) increase the number of sexual approaches.

On the whole we find that during Day XXII the amount of sexual behaviour *decreased* in both cocks. Dancing is an exception and is the only instance where we can find traces of suggestive influence from one cock to another. During the next day (Day XXIII) the amount of dancing is again considerably decreased, while on this day the more serious sexual behaviour has increased in Cock II, but not significantly in Cock I.

On the whole we have to state that the *suggestive* influence was intraceable in our cases.

The element of rivalry was more clearly shown. Not only did the two cocks fight very often through the wirenetting<sup>1)</sup>, but they also interrupted each other's sexual behaviour. During Day XXII we find Cock II rushing to the fence trying to peck Cock I (and often actually succeeding in this) once when Cock I was running after g, his wings sweeping the ground, once when Cock I had copulated with h and was dancing to her afterwards, and three times when Cock I was copulating with a hen. Cock I interrupted Cock II once when he was running for f and once in the middle of his copulating with a. In all we find seven clear instances of excited interruption of the other's sexual behaviour. In all the above mentioned cases immediate fighting took place as a result of the interruption. During Day XXIII Cock II interrupted, or tried to interrupt, once when Cock I was chasing f with his wings sweeping the ground, once when Cock I had copulated with b

<sup>1)</sup> During the first day the observer interrupted the fight every time they started, the second day no interruption took place.

and was dancing to her afterwards, and eight times when Cock I was copulating. Cock I interrupted four times or tried to, when Cock II was copulating (or in attempt). In all we find 14 interruptions on this day.

The decrease of sexual behaviour caused by the presence of another cock might be due partly to the dispersion of the attention, the other cock holding part of the attention, — partly to the diminishing of the time at disposal, the fighting taking actually a good deal of time (this factor is not likely to be very important to judge by our experiments with limitation of time, Exp. 1), — and finally to the loss of energy caused by the fighting.

It does not seem unlikely that fighting gave an outlet for energy so that the tension of the sexual need was lessened by this action <sup>1)</sup>. We had opportunity to observe on different occasions how the satisfying of one need might apparently replace the satisfying of another. We thus noted on several occasions that the cock when chasing a hen for sexual purposes, she running ahead and he following, would drop the persecution and start eating greedily on coming suddenly across some food, though even his hunger evidently could not be great as he had generally had plenty to eat just before the incident. If the sexual need was not satisfied, the hen escaping in some way, it also happened that the cock, especially Cock II, started crowing violently, so giving a spontaneous impression that this activity was an outlet for the non-satisfied sexual energy.

It cannot be discussed here whether this leading of the energy of a need into other channels can be considered as a parallel to human sublimation. But evidently other activities or other directions of the attention are able to place the sexual need in the back-ground for shorter or longer times. A man very busy at work, or solving some serious problem, does not feel any sexual need.

#### 9. *Effect of thwarting other needs*

In order to see what influence lack of food would exert on the sexual behaviour of the cock, for two days (Day XXIV and

<sup>1)</sup> I. Hermann in op. cit. p. 19 states that in apes aggressive behaviour will at times substitute for sexual behaviour, especially when the female is demanding too much sexuality from the male.

XXV) we only fed him a little at night before putting him into the dark-room (*Exp. 10*). As the cock would always be able to find food left in the ground of the great hen-run, we had to take him away from this and to leave him in the small isolation-pen. Here he was surrounded by four hens at a time; to keep him from partial over-satisfaction (p. 217 f.) the hens were changed every hour in the morning and in the afternoon, every half-hour in the evening, in the way that all the hens (except m) spent  $2\frac{1}{2}$  hours each with him, this time being distributed between the different parts of the day.

These days and the following normal day gave the following result:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XXIV	5	1	7	16	19	25
„ XXV	2	2	5	10	17	25
„ XXVI	22	38	10	4	10	25
(Normal	29	21	6	8	20	31)

The interesting feature of this table is the greatly reduced numbers of all that is not serious sexual behaviour. The weak impulses and the dancing, all that one might call „luxury”, „Überschuss”, is left out (the dancing deficit being made up for next day). At the same time the number of „catching by the neck” is slightly increased especially on the first of the days. While observing him we got the impression, as was the case during exp. 4, that he had not energy enough to continue the act.

It might be natural to ascribe this lack of surplus or lack of energy to the fact that *new* energy in form of food was not supplied during these days. To check this hypothesis we left the cock in the isolation pen during Day XXVII, changing the hens just as during Day XXIV and Day XXV (*Exp. 11*), but this time letting him have plenty of food of all kinds. During this day he manifested the following sexual behaviour:

	Imp.	Danc.	Foot	Catch	Att.	Cop.
Day XXVII	9	3	4	6	19	11

We find the same decrease in „weak impulse” and „dancing” as during the hunger days, and in addition to this also a decrease in copulations. This gives us evidence that the hunger is not the only reason for the decreased activity during Day XXIV and XXV. The lack of food may play a certain role, but certainly other factors seem to be just as important, if not more.

When the cock was enclosed in the isolation pen (Day XXIV, XXV and XXVII) his *freedom of movement* was greatly diminished, his *need of freedom* was thwarted besides his need of food. This need of freedom seems only to be a relative one. The cock and the hens are used to have a certain space within which to move freely about, but all the time surrounded by fences keeping them from getting outside their territory. The hens and the cock during the whole observation period rarely tried to get out of this territory even when the door happened to be left open. The cock sometimes tried to get out when left alone with the second family on seeing his first family in the other henrun, or when he was separated for other reasons from some of his own hens (exp. 3, 4, 6 and 8). And vice versa the hens would try to get back to their own territory and the cock when transferred to other places. But when the family was gathered in their territory, they did not show any sign of objection towards being enclosed within certain limits.

The case was quite different when they were placed in a *smaller* territory, the isolation pen. The cock or one of the hens was then continuously trying to get out. They walked up and down restlessly, ran around searching holes in the fence or trying to force themselves through the meshes in the netting. Even if most of the hens inside the pen kept quiet there would always be someone trying to get out.

The need for freedom, for freedom within the limits to which they were accustomed, seemed to be very strong, *so* strong that the reduced sexual activity, as Day XXVII shows, seems due quite as much to the thwarting of *this* need as to the thwarting of the need for food.

The need for freedom within accustomed limits seems to be strong also in other animals and it is decisive in human beings. The difficulty of breeding wild animals in zoological gardens may perhaps be due to this factor as well as to climatic factors. Imprisonment as a form of punishment is based on this need; even

if prisons are very sound and comfortable, they are felt as punishment chiefly because the prisoners are not allowed to move away whenever they like to. As long as a child knows that it *can* get out of the room when it likes to, it is keeping quiet; as soon as it knows the door to be locked, it becomes restless, kicks at the door, screams etc.

In society *laws* place certain limits on the personal freedom of the individuals. When we are used to the laws we hardly object to them. But new laws diminishing the personal freedom without the individual's consent can raise the emotions to a very high degree (Prohibition of alcohol in U.S.A., Finland, Norway, Iceland etc.). When we are not allowed to speak freely of our personal opinions or when our writings are changed or repressed by laws or by other people, our need for freedom is thwarted, the limits are drawn closer than we are used to and agree to, and as a consequence strong emotions arise.

What influence thwarting of other needs (food, freedom, work) have on the human sexual behaviour is still unknown. We know from the Great War that when the need of security was thwarted, the sexual need did not become tense until the lack of security had become habitual: Soldiers did not feel the need for sexual relations during their first months at the front even when accustomed to regular satisfaction beforehand. Contrary, unemployed are said to suffer from an increased need for sexual relations during the first month or so of unemployment, when their outlet of energy in work has been stopped, while the need tension is decreasing later on.

#### V. CONCLUSION

Regarding the „food appetite”, the amount and kind of food consumed, Katz has already pointed out how all this depends not only on the *inner* tension of the need, but also on factors *outside* the organism<sup>1)</sup>. The same thing may be said to hold good in the sexual appetite as well as in the degree and the ways in which this will be satisfied: This all depends not only on the inner tension of the need, but also on factors outside the organism, such as surroundings, amount of opportunities, the novelty of the

<sup>1)</sup> D. Katz: Op. cit. p. 2 (b).

stimuli, the „Aufforderungscharakter” of the stimuli, other centres of the attention etc.

I cannot finish this paper without placing on record my deep gratitude to Professor D. Katz, who initiated the experiments and has been following them with a great interest from day to day, nor without giving my best thanks to Professor T. H. Pear who has so obligingly furnished me with the opportunity to perform the experiments, as well as for his sincere and unceasing goodwill. I also wish to express my thanks to him for admitting me to the Psychological Laboratory of Manchester University.

#### SUMMARY

The studies here presented have as their main object the investigation of a hypothesis posed by Prof. D. Katz, to the effect that on the whole the same laws are valid for all the different needs, and that the strength of a need (or the „appetite” of the need) depends on two different factors, one interior component and one exterior. The need here studied is the sexual one, the subjects being hens and cocks, 2 families of 13 hens each, and 2 cocks.

Before the experiments could take place the usual conditions in the social and the sexual life had to be studied. The „pecking law” found by Th. Schjelderup-Ebbe is verified, the hens’ and the cock’s average sexual behaviour, its daily rhythm, the cock’s preferences among the hens etc. are studied. It is confirmed that no correlation exists between the rank order of the hens in the pecking list and in the preference list. Individual differences in the hens and the cocks are studied at the same time. The influence of weather and temperature on the sexual behaviour is found to be insignificant.

The experiments are not complete enough to give results of general validity. However, the following *tendencies* seem to occur:

1. When the sexual need is not satisfied, its energy (the „appetite”) seems to be stored; when normal opportunity to satisfy the need is again provided, the activity is correspondingly increased until the loss is approximately compensated.
2. The daily rhythm might to a certain degree be changed, but when under free conditions again, the original rhythm seems to be regained after a while.

3. The sexual appetite seems to be increased when the number of opportunities is increased. This, however, is not necessarily true during any length of time; the ordinary habits may be regained when the situation is no longer new. No special investigation is made on this last point.

4. When the strength of the need is increasing, the demand for quality is decreasing.

5. When the cock is admitted only to a few hens, partial over-satisfaction will take place, so that for some time he seems disgusted with these hens, even when they formerly used to be his favourites.

6. The addition of new stimuli (2 new hens) gave no clear results. The sexual appetite did not seem to increase, but several factors are here in play which might interfere with the sexuality (the fighting between the old family and the new hens, etc.)

7. Frequent changing of the stimuli (alternating the two families surrounding the cock) seems to increase the sexual appetite, both when a new family is offered, and still more when the old family is brought back again.

8. The close presence of another cock does not increase the sexual appetite. No suggestion from one to the other seems to take place. On the contrary, great jealousy is evident, one cock trying to interfere in the other's sexual behaviour. Much energy is used in fighting so that the energy used for sexual purposes is decreased.

9. When the needs for freedom and for food are thwarted the sexual appetite is decreased. This is also the case when the need for food is satisfied, the need for freedom still being thwarted.

On the whole, the tendencies found by D. Katz in the need for food are found also in our investigations on the sexual need.

#### RÉSUMÉ

Ces études ont pour objet d'examiner l'hypothèse de M. le Professeur D. Katz, que les mêmes lois régissent en général tous les besoins, et que la satisfaction d'un besoin dépend de deux facteurs, l'un interne et l'autre externe. Le besoin que j'ai étudié est le besoin sexuel. Les recherches ont porté sur deux familles de *Gallus domesticus*, composées chacune de 1 coq et de 13 poules.

La partie expérimentale du travail est précédée d'une partie descriptive qui s'occupe des rapports habituels entre les relations sociales et

les sexuelles. Les lois du becquage de Schjelderup-Ebbe ont été confirmées; les relations sexuelles du coq et des poules, leur rythme quotidien ainsi que la préférence du coq pour les diverses poules ont été étudiées. Entre l'ordre du becquetage et la préférence des poules par le coq, il n'existe aucune corrélation. On a étudié les différences individuelles entre les poules et les coqs. Ni le temps ni la température n'ont d'influence sur les rapports sexuels.

Les expériences ne sont pas assez complètes pour donner des résultats d'une importance générale. Pourtant, il semble que les tendances suivantes existent:

1. Si le besoin du coq n'est pas satisfait, il semble que l'appétit sexuel augmente; si alors une nouvelle occasion de satisfaire ce besoin se présente, l'activité du coq augmente proportionnellement jusqu'à ce qu'un équilibre approximatif soit établi.

2. Le rythme sexuel quotidien se transforme jusqu'à un certain degré, mais si les conditions normales se rétablissent, l'ancien rythme sexuel se rétablit aussi après quelque temps.

3. L'appétit sexuel semble augmenter avec le nombre de poules, mais cette augmentation ne devrait pas durer longtemps; si la situation ne paraissait plus nouvelle, l'ancien rapport sexuel pourrait se rétablir. Ce dernier point n'a pas été spécialement étudié.

4. En même temps que la force du désir sexuel augmente, les préentions qualitatives diminuent.

5. S'il y a peu de poules à la disposition du coq, celui-ci arrive à en être rassasié. Ces poules ont donc pendant quelque temps moins d'attrait pour lui, même si elles ont été précédemment ses poules favorites.

6. L'augmentation de la stimulation sexuelle (addition de deux nouvelles poules) n'a pas donné clairement un résultat. L'appétit sexuel n'a pas semblé augmenter; mais il y avait en jeu plusieurs facteurs qui exerçaient leur influence sur la sexualité. (Combats entre les poules de l'ancienne et de la nouvelle famille, etc.)

7. Les modifications de la présence des stimulants (échanges entre les deux familles qui entourent le coq) semblent augmenter l'appétit sexuel, et ceci a lieu aussi bien si le coq est transporté dans une nouvelle famille que s'il est ramené dans l'ancienne.

8. L'appétit sexuel d'un coq n'est pas augmenté par la présence d'un autre coq. La vue de l'autre coq ne semble avoir d'effet stimulant sur aucun d'eux. Au contraire, il se manifeste une forte jalousie qui fait que chacun des coqs essaye de troubler l'acte sexuel de l'autre. Ils dépensent beaucoup d'énergie dans leurs combats, de sorte que l'énergie disponible pour l'acte sexuel se trouve diminuée.

9. Si le désir de liberté de mouvements et de nourriture n'est pas satisfait, l'appétit sexuel diminue; il en est de même quand la faim est rassasiée, mais que le désir de liberté reste inassouvi. En somme, les tendances trouvées par M. le Professeur D. Katz au sujet de la satisfaction du besoin de nourriture ont été également observées dans nos recherches sur les besoins sexuels.

## ZUSAMMENFASSUNG

Die vorliegenden Studien haben als ihr Hauptziel, die von Prof. D. Katz aufgestellte Hypothesen zu prüfen, dass im grossen ganzen dieselben Gesetze für alle Bedürfnisse gelten und dass die Auswirkung eines Bedürfnisses von zwei Faktoren abhängt, einem inneren und einem äusseren. Das von mir untersuchte Bedürfnis ist das Sexualbedürfnis. Die Untersuchung wurde an 2 Familien von Gallus domesticus durchgeführt, jede bestehend aus 1 Hahn und 13 Hennen.

Dem experimentellen Teil der Arbeit geht ein beschreibender voran, der sich mit den gewöhnlichen Verhältnissen des sozialen und sexuellen Verhaltens beschäftigt. Schjelderup-Ebbes Gesetze der Hackliste wurden bestätigt, das sexuelle Verhalten des Hahns und der Henne, dessen täglicher Rhythmus, sowie die Vorliebe des Hahns für die verschiedenen Hennen wurden studiert. Zwischen der Hackordnung und der Bevorzugung der Hennen durch den Hahn besteht keine Korrelation. Es wurden die individuellen Unterschiede zwischen den Hennen und den Hähnen studiert. Wetter und Temperatur haben auf das sexuelle Verhalten keinen Einfluss.

Die Experimente sind nicht vollständig genug, um Resultate von allgemeiner Geltung zu liefern. Doch scheinen folgende Tendenzen zu bestehen:

1. Wenn das sexuelle Bedürfnis des Hahns nicht befriedigt wird, scheint der sexuelle Hunger zu wachsen; wenn dann wieder Gelegenheit zur Befriedigung des Bedürfnisses geboten wird, wird die Aktivität des Hahns entsprechend verstärkt, bis annähernd ein Ausgleich eingetreten ist.
2. Der tägliche sexuelle Rhythmus lässt sich bis zu einem bestimmten Grad ändern, wenn aber die normalen Bedingungen wiederhergestellt werden, stellt sich auch der alte sexuelle Rhythmus nach einiger Zeit wieder ein.
3. Der sexuelle Appetit scheint mit der Zahl der Hennen zu wachsen. Doch dürfte diese Steigerung nicht beliebig lange anhalten; wenn die Situation nicht länger neu erscheint, könnte sich das alte sexuelle Verhalten wieder einstellen. Diesem letzteren Punkt ist keine besondere Untersuchung gewidmet worden.
4. Mit zunehmender Stärke des sexuellen Bedürfnisses sinken die Ansprüche qualitativ.
5. Stehen dem Hahn nur wenige Hennen zur Verfügung, so tritt Übersättigung für diese ein. Diese Hennen sind dann für einige Zeit weniger anziehend für den Hahn, auch wenn sie vorher seine Lieblingshennen gewesen sind.
6. Die Verstärkung des sexuellen Reizes (Einführung von 2 neuen Hennen) ergab kein klares Resultat. Der sexuelle Appetit schien nicht zu wachsen, es spielten aber mehrere Faktoren hinein, die die Sexualität beeinflussten (Kämpfe zwischen den Hennen der alten und der neuen Familie, u.s.w.).

7. Wechsel in der Reizsituation (Austausch der beiden Familien, die den Hahn umgeben) scheint den sexuellen Appetit zu steigern, und zwar sowohl wenn der Hahn in eine neue Familie gebracht, wie wenn er in die alte zurückgebracht wird.

8. Die sexuelle Appetit eines Hahns wird durch die Nähe eines anderen Hahns nicht verstärkt. Ihr gegenseitiger Anblick scheint auf sie nicht anregend zu wirken. Im Gegenteil, es äussert sich starke Eifersucht, der eine Hahn versucht die sexuelle Betätigung des andern zu stören. Viel Energie wird bei den Kämpfen verbraucht, sodass die für sexuelle Zwerke verfügbare Energie abnimmt.

9. Wenn das Bedürfnis nach freier Bewegung und Nahrung unbefriedigt bleibt, nimmt der sexuelle Appetit ab. Dasselbe gilt, wenn der Hunger nach Nahrung befriedigt wird, aber des Verlangen nach Freiheit unbefriedigt bleibt.

Im ganzen gilt, dass die Tendenzen, die D. Katz bei der Befriedigung des Nahrungsbedürfnisses gefunden hat, auch bei unseren Untersuchungen des sexuellen Bedürfnisses zu beobachten sind.